THURSDAY, MAY 25, 1882

CHARLES DARWIN [1]

II.

NO man of his time has exercised upon the science of Geology a profounder influence than Charles Darwin. At an early period he took much interest in geological studies, and all through life, while engaged in other pursuits, he kept himself acquainted with the progress that was being made in this department of natural knowledge. His influence upon it has been twofold. It arises partly from the importance and originality of some of his own contributions to the literature of the science, but chiefly from the bearing of his work on other branches of natural history.

When he began to direct his attention to geological inquiry the sway of the Cataclysmal school of geology was still paramount. But already the Uniformitarians were gathering strength and, before many years were past, had ranged themselves under the banner of their great champion Lyell. Darwin, who always recognised his indebtedness to Lyell's teaching, gave a powerful impulse to its general reception by the way in which he gathered from all parts of the world facts in its support. He continually sought in the phenomena of the present time the explanation of those of the past. Yet he was all the while laying the foundation on which the later or Evolutional school of geology has been built up.

Darwin's specially geological memoirs are not numerous, nor have they been of the same epoch-making kind as his biological researches. But every one of them bears the stamp of his marvellous acuteness in observation, his sagacity in grouping scattered facts, and his unrivalled far-reaching vision that commanded all their mutual bearings, as well as their place in the general economy of things. His long travels in the *Beagle* afforded him opportunities of making himself acquainted with geological phenomena of the most varied kinds. With the exception of one or two minor papers written in later years, it may be said that all his direct contributions to geology arose out of the *Beagle* voyage. The largest and most important part of his geological work dealt with the hypogene forces of nature—those that are concerned in volcanoes and earthquakes, in the elevation of mountains and continents, in the subsidence of vast areas of the sea-bottom, and in the crumpling, foliation, and cleavage of the rocks of the earth's crust. His researches in these subjects were mainly embodied in the "Geology of the Voyage of the *Beagle*"—a work which, in three successive parts, was published under the auspices of the Lords of the Treasury.

The order chosen by Darwin for the subjects of these three parts probably indicates the relative importance with which they were regarded by himself. The first was entitled "The Structure and Distribution of Coral Reefs" (1842). This well-known treatise, the most original of all its author's geological memoirs, has become one of the recognised classics of geological literature. The origin of those remarkable rings of coral-rock in mid-ocean had given rise to much speculation, but no satisfactory solution of the problem had been proposed. After visiting many of them, and examining also coral-reefs fringing islands and continents, he offered a theory which for simplicity and grandeur strikes every reader with astonishment. It is pleasant after the lapse of many years to recall the delight with which one first read the "Coral Reefs," how one watched the facts being marshalled into their places, nothing being ignored or passed lightly over, and how step by step one was led up to the grand conclusion of wide oceanic subsidence. No more admirable example of scientific method was ever given to the world, and even if he had written nothing else, this treatise alone would have placed Darwin in the very front of investigators of nature.

The second part was entitled "Geological Observations on the Volcanic Islands visited during the voyage of H.M.S. *Beagle*, together with some brief notices on the geology of Australia and the Cape of Good Hope" (1844). Full of detailed observations, this work still remains the best authority on the general structure of most of the regions it describes. At the time it was written, the "Crater of elevation theory," though opposed by Constant Prevost, Scrope, and Lyell, was generally accepted, at least on the Continent. Darwin, however, could not receive it as a valid explanation of the facts, and though he did not adopt the views of its chief opponents, but ventured to propose a hypothesis of his own, the observations impartially made and described by him in this volume must be regarded as having contributed towards the final solution of the question.

The third and concluding part bore the title of "Geological Observations on South America" (1846). In this work the author embodied all the materials collected by him for the illustration of South American geology save some which had already been published elsewhere. One of the most important features of the book was the evidence which it brought forward to prove the slow, interrupted elevation of the South American continent during a recent geological period. On the western sea-board he showed that beds of marine shells could be traced more or less continuously for a distance of upwards of 2000 miles, that the elevation had been unequal, reaching in some places at least to as much as 1300 feet, that in one instance at a height of 85 feet above the sea, undoubted traces of the presence of man occurred in a raised-beach, and hence that the land had there risen 85 feet since Indian man had inhabited Peru. These proofs of recent elevation may have influenced him in the conclusion which he drew as to the marine origin of the great elevated plains of Chili. But at that time, there was a general tendency among British geologists to detect evidence of sea-action everywhere and to ignore or minimise the action of running water upon the land. An important chapter of the volume, devoted to a discussion of the phenomena of cleavage and foliation, is well known to every student of the literature of metamorphism.

The official records of the *Beagle* did not, however, include all that Darwin wrote on the geology of the voyage. He contributed to the *Transactions* of the Geological Society (vol. v. 1840) a paper on the connection of volcanic phenomena. In the same publication (vi. 1842) appears another, on the erratic boulders of South

[1] Continued from p. 51.

America; while a third, on the geology of the Falkland Islands, was published later.

While dealing with the subterranean agents in geological change, he kept at the same time an ever watchful eye upon the superficial operations by which the surface of the globe is modified. He is one of the earliest writers to recognise the magnitude of the denudation to which even recent geological accumulations have been subjected. One of the most impressive lessons to be learnt from his account of Volcanic Islands is the prodigious extent to which they have been denuded. As just stated he was disposed to attribute more of this work to the action of the sea than most geologists would now admit; but he lived himself to modify his original views, and on this subject his latest utterances are quite abreast of the time. It is interesting to note that one of his early geological papers was on the Formation of Mould (1840), and that after the lapse of forty years he returned to this subject, devoting to it the last of his volumes. In the first sketch we see the patient observation and shrewdness of inference so eminently characteristic of the writer, and in the finished work (so recently noticed in these columns) the same faculties enriched with the experience of a long and busy life. In bringing to light the operations of the earthworm, he called the attention of geologists to an agency, the real efficiency of which they probably do not yet appreciate. Élie de Beaumont looked upon the layer of grass-covered soil as a permanent datum-line from which the denudation of exposed surfaces might be measured. But, as Darwin showed, the constant transference of soil from beneath to the surface, and the consequent exposure of the materials so transferred to be dried and blown away by wind, or to be washed to lower levels by rain, must tend slowly but certainly to lower the level even of undisturbed grass-covered land.

To another of his early papers reference may be made, from its interest in the history of British geology. Buckland, following in the footsteps of Agassiz, had initiated that prodigious amount of literature which has now been devoted to the records of the Glacial period in this country, by reading to the Geological Society a paper "On Diluvio-glacial Phenomena in Snowdonia and in Adjacent Parts of North Wales" (1841). Darwin, whose wanderings in South America had led him to reflect deeply upon the problems presented by erratic blocks, took an early opportunity of visiting the Welsh district described by Buckland, and at once declared himself to be a believer in the former presence of glaciers in Britain. His paper (1843) in which this belief is stated and enforced by additional observations, stands almost at the top of the long list of English contributions to the history of the Ice Age.

The influence exercised upon the progress of geology by Darwin's researches in other than geological fields, is less easy to be appraised. Yet it has been far more widespread and profound than that of his direct geological work. Even as far back as the time of the voyage of the *Beagle*, he had been led to reflect deeply on some of Lyell's speculations upon the influence of geological changes on the geographical distribution of animals. From that time the intimate connection between geological history and biological progress seems to have been continually present in his mind. It was not, however, until the appearance of the "Origin of Species" in 1859 that the full import of his reflections was perceived. His chapter on the "Imperfection of the Geological Record" startled geologists as from a profound slumber. It would be incorrect to say that he was the first to recognise the incompleteness of the record; but certainly until the appearance of that famous chapter the general body of geologists was blissfully unconscious of how incredibly fragmentary the geological record really is. Darwin showed why this must necessarily be the case; how multitudes of organic types, both of the sea and of the land, must have decayed and never have been preserved in any geological deposit; how, even if entombed in such accumulations, they would in great measure be dissolved away by the subsequent percolation of water. Returning to some of his early speculations he pointed out that massive geological deposits rich in fossils, could only have been laid down during subsidence, and only where the supply of sediment was sufficient to let the sea remain shallow, and to entomb the organic remains on its floor before they had decayed. Hence, by the very conditions of its formation, the geological record, instead of being a continuous and tolerably complete chronicle, must almost necessarily be intermittent and fragmentary. The sudden appearance of whole groups of allied species of fossils on certain horizons had been assumed by some eminent authorities as a fatal objection to any doctrine of the transmutation of species. But Darwin now claimed this fact as only another evidence of the enormous gaps in geological history. Reiterating again and again that only a small fraction of the world had been examined geologically and that even that fraction was still but imperfectly known, he called attention to the history of geological discovery as furnishing itself a strong argument against those who argued as if the geological record were a full chronicle of the history of life upon the earth. There is a natural tendency to look upon the horizon upon which a fossil species first appears as marking its birth, and that on which it finally disappears as indicating its extinction. Darwin declared this assumption to be "rash in the extreme." No palæontologist nor geologist will now gainsay this assertion. And yet how continually do we still hear men talking of the stages of the geological record, as if these were sharply marked off everywhere by the first appearance and final disappearance of certain species. The boldness with which Darwin challenged some of these long-rooted beliefs is not less conspicuous than the modesty and deference with which his own suggestions were always given. "It is notorious," he remarked, "on what excessively slight differences many palæontologists have founded their species; and they do this the more readily if the specimens come from different sub-stages of the same formation."

Starting from this conception of the nature of the geological record, Darwin could show that the leading facts made known by palæontology could be explained by his theory of descent with modification through natural selection. New species had slowly come in, as old ones had slowly died out. Once the thread of succession had been broken it was never taken up again; an extinct species or group never reappeared, yet extinction was a slow and unequal process, and a few descendants of

ancient types might be found lingering in protected and isolated situations. "We can understand how it is that all the forms of life, ancient and recent, make together one grand system; for all are connected by generation. From the continued tendency to divergence, the more ancient a form, is the more generally it differs from those now living. The inhabitants of each successive period in the world's history have beaten their predecessors in the race for life, and are in so far, higher in the scale of nature; and this may account for that vague, yet ill-defined sentiment, felt by many palæontologists, that organisation on the whole has progressed. If it should hereafter be proved that ancient animals resemble to a certain extent the embryos of more recent animals of the same class, this fact will be intelligible."

Again, what a flood of fresh light was poured upon geological inquiry by the two chapters on Geographical Distribution in the "Origin of Species!" A new field of research, or, at least, one in which comparatively little had been yet attempted, was there opened out. The grouping of living organisms over the globe was now seen to have the most momentous geological bearings. Every species of plant and animal must have had a geological history, and might be made to tell its story of the changes of land and sea.

In fine, the spirit of Mr. Darwin's teaching may be traced all through the literature of science, even in departments which he never himself entered. No branch of research has benefited more from the infusion of this spirit than geology. Time-honoured prejudices have been broken down, theories that seemed the most surely based have been reconsidered, and, when found untenable, have been boldly discarded. That the Present must be taken as a guide to the Past, has been more fearlessly asserted than ever. And yet it has been recognised that the present differs widely from the past, that there has been a progress everywhere, that Evolution and not Uniformitarianism has been the law by which geological history has been governed. For the impetus with which these views have been advanced in every civilised country, we look up with reverence to the loved and immortal name of Charles Darwin.

(*To be continued.*)

THE TOTAL ECLIPSE

THE Special Correspondent of the *Daily News* with the English Eclipse Expedition telegraphs as follows under date, Sohag, May 17:—

This eventful morning was the finest we have yet had, cool and without a cloud. A great crowd of natives in picturesque costumes lined the road and the hill between the camp and Sohag. The shore of the Nile, except before the observatories, was packed with dahabeeahs bringing the governors of the provinces and other notables to observe the eclipse and do honour to the strangers. Thanks to Moktar Bey, in charge of the camp, and a force of soldiery, there was no confusion. Along a line of 300 yards the French, English, and Italian observers were left in undisturbed possession of tents and observatories. Nevertheless, while the sky darkened and assumed a leaden hue, the hills bounding the Nile bathed in purple, the great silence gave way, and from river and palm-shaded slope arose a shout of wonder and fear, which reached its climax at the moment of the sun's disappearance; nor ceased then, for, in addition to the horror of an eclipse—which the natives here as in India, attribute to the act of a dragon—there appeared in the heavens on the right of the sun an unmistakable scimetar. The eclipse had, in fact, revealed the existence of a new comet. Despite the short totality, many valuable results have been obtained. I am permitted to send a copy of the collective telegram sent to the various Governments, showing many new facts touching the sun's atmosphere; though matters have not become much simpler, which means more work. The layer to which much absorption has been ascribed seems vanishing from existence. The band K in the spectrum of the corona fully explains the eclipse colouring. The collective note is as follows:—

"Unprecedented facilities have been accorded by the Egyptian Government for the observation of the eclipse. A plan was agreed upon between the English, French, and Italian expeditions. Among the results, the most satisfactory are photographs of the corona, and a complete spectrum obtained by Schuster on Abney's plates. H and K are the most intense lines. A study of the red end of the spectrum of corona and protuberances was made by Tacchini. A comet near the sun was a striking object; it was photographed and observed by the naked eye. Bright lines were observed before and after totality at different heights by Lockyer, with intensities differing from Fraunhofer's lines; by Lockyer and Trépied an absolute determination was made of the place of the coronal line 1474 in Kirchhoff's scale; by Thollon and Trépied the absence of dark lines from the coronal spectrum was noted. Tacchini and Thollon, with very different dispersions, noted many bright lines in the violet. Thollon observed spectrum of the corona, and Schuster photographed it. The hydrogen and coronal line were studied in the grating spectroscope by Buisieux, and with direct vision prism by Thollon. Rings were observed in the grating by Lockyer, of the first, second, and third order. The continuous spectrum is fainter than 1878, stronger than 1871. An intensification of the absorption lines was observed in group B, at moon's edge, by Trépied and Thollon.—(Signed), LOCKYER, TACCHINI, and THOLLON."

When our cases are packed, we shall start directly home.

Captain Abney writes as follows to the *Photographic News*:—

I have received a brief telegram from Egypt regarding the Eclipse Expedition, and as it is in cipher I give the gist of the news. "Very successful all round. The whole of the spectrum with blue lines on a continuous background has been photographed. Prominences photographed with the prismatic camera (showing, of course, ring spectrum). Three photographs taken of the corona. A comet close to sun photographed with the prismatic and also ordinary cameras."

A telegram from the Alexandria correspondent of the *Daily News* states that Mr. Lockyer was to leave for London yesterday by the Peninsular and Oriental Company's steamer *Clyde*, while the other members of the Eclipse Expedition, with their instruments, were to leave next week.

KANT'S CRITIQUE OF PURE REASON

Immanuel Kant's Critique of Pure Reason. In Commemoration of the Centenary of its First Publication. Translated into English by F. Max Müller. With an Historical Introduction by Ludwig Noiré. Two Vols. (London: Macmillan and Co., 1881.)

THE records of science and philosophy during the past few years have been especially fertile in indications of a desire to place the relations of these two departments of inquiry upon a better footing than that of their former history. The desire has its source not in a spirit of concession but in a consciousness of necessity. A deeper criticism of conceptions with which in scientific investigation it is not possible to dispense, has brought several of its chief apostles face to face with fundamental obscurities and even contradictions which seem to cast doubt upon the validity of these conceptions. On the other hand philosophy has of late been coming into extensive contact with results obtained by scientific methods, and has been compelled either to modify its position, or go to the wall. The result is that attention has been increasingly directed to that critical examination of the nature of human knowledge, which claims on its negative side to have finally destroyed the old metaphysics and assigned definite limits to investigation, on its positive side to have exhibited these limits as arising out of the ultimate constitution of mind. The translation, just published, of the "Kritik der reinen Vernunft," is one of the latest contributions to the literature of this subject. The cry of "Back to Kant" which has of late years been heard so frequently in this country and abroad, has been responded to by Prof. Max Müller with two well-appointed volumes. Of these the first contains the translator's preface, an "Historical Introduction" by Prof. Noiré, and a translation of those passages of the second edition of the "Kritik," which differ from the corresponding passages in the first. The second volume consists of the translation of the first edition. The merits of the introductions and translations will be best estimated after the consideration—as far as the compass of a review will allow—of Kant's position.

To understand the critical philosophy, it is essential to realise that its problem and subject-matter are entirely different from any thing that is or can be dealt with by science in the ordinary acceptation of the term, and in particular from the investigations of physiological or other psychology. Science deals with what it is customary in our aspect to call mind, and in another cerebral organisation, and inquires into the relations of this to the surrounding environment. It seeks to lay bare the mechanism of perception and ideation, and to exhibit the complete dependence of mental upon cerebral functions. And of late years it has pretty well justified its title to the exclusive occupation of the field as against the old introspective psychology. Mind and its environment are alike the objects of and given in what may be indifferently spoken of as knowledge, consciousness, or experience. That is to say, they presuppose knowledge (to use the appellation which is perhaps least encumbered with question-begging associations) as that through which, like everything else, they exist, and in which the meaning of existence is to be found. The old Berkeleian reduction of *esse* to *percipi* is matter of common knowledge, and the leaders of scientific thought show a very proper disposition to treat it as a truism. For the statement that the universe in ultimate analysis is reducible to a succession of states or groups of states of consciousness, amounts to no more than the statement that the universe exists, and may be dismissed as outside the region of scientific questions in exactly the same sense as is this assumption. But if the step from Berkeley to Hume be taken, and existence regarded as the "impressions and ideas" of a particular individual, whose consciousness itself exists only so far as it is the object of knowledge, there ensue logical consequences of the gravest description. The inquirer is then confronted with the conclusion that the universe in so far as real is nothing more than an arbitrary sequence of phases of his own mind, as to which there is not the remotest reason for believing that the uniformity of the past will be resembled in the events of the future. Scientific and indeed all propositions, particular as well as general, become a delusion and self-consciousness an unintelligible deception. Since Hume's "Treatise" was published, it has been characteristic of his would-be interpreters, until within the last few years, to misunderstand him, of scientific men to ignore him, and of that succession of distinguished writers who have sought to apply the canons of scientific method to the problems of philosophy, in a somewhat perplexing fashion to do both. Of late the significance of Hume's teaching has been better understood. Men have come to see that if reality consists in ultimate analysis of a succession of sensations which, existing only in so far as they are felt, cannot be connected excepting by a purely subjective process, they must accept the logical consequences that not only is the belief in a uniform constitution of nature no longer tenable, but that the subjective semblance of such a belief is as incapable of being accounted for as the fact itself. This was the teaching of Kant, and those who seek its detailed justification and the proof that Hume did more than show the unreliability of general propositions, will do well to turn to the pages of the late Mr. Green and of Mr. Arthur Balfour. It is characteristic of Kant, that although he grasped the serious and self-destructive character of Hume's conclusion as to the impossibility of knowledge much more fully than its originator, he yet speaks of it as though it were of importance, only because it detracted from the supposed necessary truth of mathematical and causal relations. He has accordingly misled the majority of his critics into the unfortunate idea, that in denying the necessity of these relations, they have displaced the foundation of the critical system. The problem stood thus. It was clear that existence had no meaning except the being perceived by an actually or possibly percipient consciousness; and the only known form of such a consciousness was the individual self. But to say that existence meant the being a mode of the consciousness of the individual self, involved the contradiction of facts by the implicit denial of the possibility of even a semblance of knowledge. There was only one alternative: to recognise that the self in which the meaning of existence was to be sought was not the finite self disclosed in experience—an apparent point in a boundless expanse, from which it was distinguished only by the fact of its being always "here and now," but constructive

thought, which was always subject and never object in knowledge, of which it was wrong to predicate existence, because it was above the categories of existence in that only as its object could things be said to be. For Kant, such an intellectual activity was something very different from that "unknowable" of which so much has been written. Of the "unknowable" it may be said, that although it exists, is a cause and so forth, it can never be intelligible to a finite mind, but it is none the less the object as distinguished from the subject of thought.

Kant's (as the time went) great knowledge of physical science no doubt contributed to cause him to revolt keenly against Hume's apotheosis of the individual self. He had anticipated, and to a surprising degree grasped, the modern conception of evolution. He had worked out, independently of Laplace, the mechanical theory of the solar system, and had enunciated the hypothesis of development in the organised world. For him there was no possibility of supernatural interference, and Man was but the last link in a gradually evolved chain of life. He could not assent to conclusions which assigned to an individual consciousness—itself but a point in the boundless immensity of space and time—the position of being the foundation of the whole phenomenal universe, and which regarded knowledge as a fiction. He saw clearly enough that the problem was not an ordinary scientific problem of relations within experience, but the problem as to the constitution of experience itself. In science (as indeed in metaphysics) we are always concerning ourselves with some conceivable object of knowledge, and we assume that there is no question about the conditions of that experience in which that object is actually or conceivably included as a part. But Kant's problem was that of knowledge itself, with the relations of space, time, causality, &c., which enter into its constitution, and which, as the conditions of the possibility of objects of knowledge could bear themselves, are implied in such objects. His method was that which is the general method of inductive reasoning, to apply an hypothesis to certain data, and to modify it, as appeared necessary from the result of the test of adequacy to the explanation of these data. His findings were in outline these: Berkeley and Hume showed that things cannot create thought, or exist otherwise or in any other sense than for thought. Therefore, thought must create things. But we find an inexhaustible material in nature which cannot be understood as the product of thought—the *matter* of perception as distinguished from the formal relations which are found to be exclusively the work of thought in knowledge; this formless matter Kant declares to become the object of knowledge—that is to attain reality—in so far as it is brought under two pure *a priori* forms, which belong exclusively to mind, space, and time. But in the constitution of the real there is something more implied, for space and time, taken by themselves, are merely the *formal possibilities* of spatial and temporal arrangement. Kant now shows that the matter of perception—the raw material of sensation of which all we can say is that it is wholly meaningless and without reality, excepting as thought makes it otherwise—is determined in the two pure forms of perception in the fundamental relations which he terms "categories," and which include not only quantity, quality, substance, cause and effect, &c., but every other relation of experience. The main difficulty in understanding Kant arises from the tendency to forget that the process of creation, which has just been in outline indicated, is not a process taking place in space and time. It is a process of pure thought which can never be made the object of knowledge, because, as has been already stated, knowledge with its distinction between subject and object implies, these very spatial, temporal, and other relations which are themselves logical elements in the process. Such thought can never be the property of an individual organism, completely dependent on what surrounds and has been before it. The finite self cannot be taken to explain the process through which, like the rest of existence, it is created. In making itself its own object thought is presented as an individual, limited like other individuals and conditions, within the field of experience. We only grasp Kant's meaning when we realise that by the thought which he finds to be creative of the objective universe, he does not mean the mind of an individual, but an intellectual activity which cannot itself become an object of knowledge, because in it and by it is created the very distinction between subject and object. Thought in this sense is pre-supposed by and is logically prior to all existence. Since it can operate in its construction of the unformed manifold of sensation into reality, only in the forms of space and time, reality is limited in its possibilities to what can be represented as existent in space and time; and from this it follows that knowledge is limited by imagination. But though our reasoning is thus only valid in so far as it is confined to actual or possible experience, thought has still, according to Kant, a power of extending itself by means of the categories alone beyond these limits, a procedure which leads to inevitable contradictions when an attempt is made to apply conclusions reached in this way to experience. It is just here that Kant's teaching becomes of interest to science, for these contradictions are the very ultimate difficulties of science, about which so much has been said of late. Kant discusses them at great length, and reference may be here made, by way of illustration, to his solution of the difficulty in the conception of the atom. In actual *experience* we cannot meet with, or in possible *experience* imagine an atom that is not of finite dimensions. Yet *reasoning* without reference to experience leads us to the inevitable conclusion, that whatever is of finite dimensions is further divisible *ad infinitum*. We predicate of the atom simultaneously that it is, and is not of finite dimensions. But in the first case we mean a conceivable object of perception in space; in the second, an unrealisable conception of thought from which no valid inference can be drawn as to reality. The two sorts of knowledge are wholly distinct, and hence their apparently contradictory results are not real contradictions. The difficulty arises not from mistaken scientific reasoning, but from the intrinsic nature of knowledge itself.

Between the representations of the relations of matter in space and time and the figments of abstract *à priori* reasoning, Kant goes on to show that there is an intermediate operation of thought, which, while it does more than create mere figments, yet does not create the real, although it modifies it. Its results are exemplified in those aspects under which the world is presented as beautiful or the reverse, and as organised. Organisation,

for example—the characteristic of which may roughly be said to be that the whole determines its parts—is a species of relation which is unreal, in that it cannot be represented as a fact in space and time. For *quâ* space and time what we call and must think of as an organised whole, is merely a mechanical aggregate of parts which are external to, and independent of each other. Yet the knowledge of nature implies that the conceptions of organisation are real in the sense that experience *suggests* and forces them upon us, and without them nature would not only seem quite different from what it is, but could not be a connected whole at all. In other words, while an aggregate of purely mechanical relations is logically conceivable, such an aggregate would necessarily be quite different from the universe as known to us. The recognition of nature as beautiful and as organised is essential to its existence as nature, and these aspects cannot be got rid of although they are not real in the sense that the mechanical aspects are real. There are thus different phases in, or kinds of knowledge, all arising out of the ultimate constitution of intelligence. This result carries with it the solution for Kant of a number of difficulties. To ask, for example, how that which is organised springs out of an environment which is not organised, is to mistake a problem of knowledge for a problem of the relation of the objects of knowledge. For there is no line of demarcation which separates the organism from its environment. We speak as if there were such a line, because, for the purposes of advancing the limited knowledge of the individuals (which, because it is conditioned by space and time, cannot comprehend the whole universe *sub specie æternitatis*), it is convenient to abstract now from one sort of relations, now from another, and to talk of things as if they presented the aspects *only* of mechanism, or *only* of organisation. Kant declared that the twofold aspect was everywhere potentially present, because of the twofold operation of thought in the constitution of things.

Whether Kant was right in his conclusion that there were different *kinds* of knowledge, or whether he ought not to have taught that there were rather different stages than kinds, this is no place to inquire. When the systems of the late German philosophical writers have been stripped of what is at the same time most prominent and most useless in them, it will be found that they contain much valuable and detailed suggestion upon this point. It may be that Kant's theory of knowledge is imperfect, and that his distinctions are in many cases artificial and unwarrantable. But his criticism forms the basis of a new departure in investigation, and it cannot be understood without being to a great extent assented to. Not the least of his achievements is that he has sifted to their foundation and placed in a new light such metaphysical abstractions of science as matter, cause, organisation and mind, and has shown why and in what sense they give rise to problems which appear insoluble. His method was intrinsically the same as that of science generally, and to him belongs the credit of having brought science and philosophy into a definite connection. Those who have best followed his teaching have most clearly understood that the future of philosophy is to be looked for, not in a slavish adhesion to Kant's or any other system, but in the detailed application of his principles, to the critical investigation of the methods of particular branches of empirical inquiry. Already the effect of such an application has been shown in the new conception of history which has resulted from it, and there are indications that the time is not far away when men of science will begin to consider the position of their special departments in the light of the theory of knowledge.

It remains to be considered how far Professors Max Müller and Noiré have succeeded in making Kant intelligible to an English-speaking public. One cannot help feeling how much better the work would have been had it consisted simply of one volume containing the translation of the first edition of the "Kritik," with that of the passages from the second edition printed in the first volume. Of Prof. Noiré's Introduction it is difficult to speak with any satisfaction. It presents just such a view of the history of philosophy previous to Kant's time as used to be current in the days of Sir William Hamilton. The author's study of philosophy has apparently been the work of his leisure moments. To suppose, as both he and Prof. Müller seem to suppose, that a further development of the theory of knowledge is to be looked for from philology, is simply to ignore Kant's distinction between knowledge as a fact of experience and as that which is constitutive of experience. As has already been pointed out, it is in the former sense only that thought can be treated as dependent upon a particular organism, and consequently as related to language. In the latter sense alone, on the other hand, is it that which is the subject of Kantian investigation. Those who desire an historical introduction to German philosophy will do well to consult the pages of Prof. Caird rather than of Prof. Noiré.

As regards the translation, the comparison of what has been recently published by Dr. Hutchison Stirling with the work of Prof. Müller is not to the advantage of the latter. No doubt the work is grammatically excellent, and the style and accuracy by far superior to that of the old translations, but it lacks that grasp of the subject which enables Dr. Stirling, in translating the first part of the "Kritik," to reproduce not merely German words by English words, but German ideas by English ideas. Yet while it may be that the "Kritik der reinen Vernunft" remains yet to be translated, this is because the reproduction in the English language of such a work must fulfil ideal requirements before it can be accepted as satisfactory. Prof. Müller has given to students of philosophy much that they did not possess before, and that is far superior to the ordinary work of this sort. His offering is indeed what he intended it to be, a fitting commemoration of the centenary of the date on which was published the treatise which was destined to revolutionise philosophy. A faithful and literal translation of that treatise is a boon for which he will not find the public ungrateful to him.

R. B. HALDANE

OUR BOOK SHELF

Insects Injurious to Forest and Shade Trees. By A. S. Packard, jun., M.D. Bulletin No. 7, United States Entomological Commission, pp. 275, 8vo. (Washington: Government Printing Office, 1881.)

THE industry and energy displayed by the United States official entomologists is astonishing, and the amount of the literature of economic entomology issued by them

would, if collected, form in itself a goodly library. One of these most valuable reports forms the subject of the present notice, closely printed, teeming with information, and illustrated by a multitude of excellent woodcuts. The amount of sound biological teaching is very great, and put forward in a manner that renders it intelligible alike to the "scientist" and to those for whose benefit it is more particularly intended. The author notices all the insects (mostly in great detail) feeding on particular trees, such as oak, elm, hickory, willow, pine, &c., &c., without special reference, in the first instance, to the particular *species* of these trees. This is a good plan, for it is only occasionally that certain insects are attached particularly to certain species in a *genus* of trees: these are specially indicated under the larger headings. We have often found ourselves in a dilemma in attempting a notice of these American reports, and this condition is strikingly in force with regard to this one in particular. Almost without exception, they are sound and lasting additions to the scientific literature of entomology; this one is especially so. But then there is the economic side of the question to be considered, and that is the most difficult. Naturally every insect that is attached to a particular species of plant, by feeding upon it, may in a certain sense be said to be "injurious" to that plant. Thus, in this present Report, under "Willow" we find even the "Camberwell Beauty" (*Vanessa antiopa*) included in the list of enemies; but we are quite sure that no one (not even the author) seriously imagines that it (with myriads of other species mentioned) is an "injurious insect" from an economic point of view. Certain insects feed on certain plants, and will eat no other; if the plant is exterminated, the insect disappears, and to keep up the balance of nature, it is quite possible that if the insect were exterminated in the first instance, some more destructive enemy (or disease) might eventually attack the plant. But the greater part of the enemies to *trees* commit their ravages by attacking the wood or bark, and here especially we think economic entomologists keep too much in the background the fact that many insects (and many of those here under consideration) act mainly the *rôle* of scavengers. Undoubtedly a leaf-feeder often attacks the most healthy trees, and as a rule it only becomes really injurious when present in extraordinary numbers; but with regard to what may be termed lignivorous insects, we strongly incline to refuse to see in the insect itself (in the majority of instances) the initial cause of the unhealthy condition; on the contrary we regard it as only stepping in to hasten decay commenced by causes quite unconnected with its presence. Our author, apparently unconsciously, virtually acknowledges this in his suggestions of remedies with regard to a beetle infesting the spruce (p. 277), and also elsewhere, by recommending, above all, preventive measures, these consisting in destroying all dead and dying trees, in which the insects especially abound. An unhealthy condition of the tree is the most favourable for the development of the beetle; but we are not of those who suppose a prescience in the latter which induces it to attack healthy trees for the benefit of prospectively remote generations of its descendants.

We wish Dr. Packard had not gone out of his way to coin worse than useless "*English*" *names*, many of which must prove more difficult to the class for whose benefit they are intended than are the scientific ones. With this exception, we thank him heartily for having produced a most valuable report. R. McLACHLAN

The Law of Kosmic Order: An Investigation of the Physical Aspect of Time. By Robt. Brown, jun. (London: Longmans, Green, and Co.).

A SHORT while ago we gave an account of the origin of the zodiacal signs so far as recent Assyrian researches enable us to determine it. Mr. Robert Brown has now published a little book on almost the same subject, the object of which is to trace the mythological conceptions to which the names given to the signs by the Accadians were due. He comes to the conclusion that the year was regarded by them as an extended nycthemeron, half the signs being diurnal or relating to the deities of day, and the other half being nocturnal, concerned with myths of the night. Early man thus recognised that there was one and the same law of "Kosmic Order" pervading all conceptions of time. In the course of his investigation Mr. Brown draws upon Egyptian and Iranian sources, but his chief materials are necessarily derived from the monuments of ancient Babylonia. Unfortunately the progressive nature of Assyrian study often renders what was written on the subject a few years ago more or less obsolete, and hence it happens that some of the statements on which he relies have been corrected or modified by subsequent research. Thus the name of the second zodiacal sign, as has already been mentioned in NATURE, meant "the directing Bull" in Accadian rather than "the propitious Bull," as Mr. Brown gives it. It is true that the word had both significations, but the signification of "propitious" was a later and derivative one. The name of the seventh sign again was "illustrious mound," not "illustrious altar," and seems to have referred to the story of the Tower of Babel, whose building was placed at the autumnal equinox, while the builder himself was called "the king of the illustrious mound." Such corrections, however, seldom, if ever, touch Mr. Brown's arguments or diminish the value of his interesting book. We can thoroughly recommend it to those who care to study a curious chapter in primitive astronomy.

Uganda and the Egyptian Sudan. By the Rev. C. T. Wilson, M.A., F.R.G.S., and R. W. Felkin, F.R.G.S. Two vols. (London: Sampson Low and Co., 1882.)

THIS double narrative is one of great interest. Mr. Wilson was one of the Church Missionary Society's missionaries sent out to King Mtesa on account of the favourable report of Mr. Stanley with regard to the eagerness of the Uganda potentate for instruction. Uganda, our readers will remember, is a district on the north and north-west of Victoria Nyanza, visited long ago by Speke, when Mtesa was quite a youth. Mr. Wilson's stay extended over two years, 1877-79. During that time, he had excellent opportunities of becoming acquainted with Uganda and the Victoria Nyanza and the districts on its south shores. He reached his destination by travelling west and north from Zanzibar, and was favourably received by Mtesa. He had much intercourse with that monarch, and gives a very rational estimate of his character, not by any means so enthusiastic as that of Mr. Stanley. Mr. Wilson's notes of his journey contain many additions to our knowledge of the region he traversed. The most important part of his narrative is that which relates to the country and people of Uganda. His chapters on Life in Uganda, on Uganda and the Waganda, and on the government and language of the Waganda, are full of fresh and interesting information, and will be valued both by ethnologists and geologists. Mr. Wilson is a favourable type of the missionary, thoroughly practical, a good observer, and a hard worker. He collected many specimens of plants, a list of which is given in the appendix, with vocabularies, and meteorological and hypsometrical observations. Mr. Felkin reached Uganda by proceeding from Suakin to Berber on the Nile, and up that river to Uganda—the first time that the Victoria Nyanza had been reached by that route. Both he and Mr. Wilson returned to Suakin by making a circuit round by the sources of the Bahr-el-Arab, and across by Obeid to the Nile. They accompanied the ambassadors sent by Mtesa to this country. Mr. Felkin's notes on the hydrography and natural history, as well as on the social and political condition of the country

traversed, are of much interest. The work is well supplied with good maps, and has a number of good and useful illustrations. It is well worth reading.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Dr. Siemens' Solar Hypothesis

I HAVE been waiting for several weeks for answers to the following rather obvious objections to Dr. Siemens' Solar Hypothesis, but I have not seen them either asked by others or answered by Dr. Siemens.

1. How the interplanetary gases near the sun acquire a sufficient radial velocity to prevent their becoming a dense atmosphere round him?

2. Why enormous atmospheres have not long ago become attached to the planets, notably to the moon?

3. Why the earth has not long ago been deluged when a constant stream of aqueous vapour that would produce a rain of more than 30 inches per annum all over the earth must annually pass out past the earth in order to supply fuel to be dissociated by the heat that annually passes the earth?

4. Why we can see the stars although most of the solar radiations are absorbed within some reasonable distance of the sun?

GEO. FRAS. FITZGERALD

40, Trinity College, Dublin, May 16

I HAVE the pleasure to reply to the very pertinent questions put by Prof. FitzGerald as follows:—

1. The gases being for the most part hydrogen and hydrogen compounds have a low specific gravity as compared with the denser gases forming the permanent solar atmosphere. On flashing into flame in the photosphere, their specific gravity would be vastly diminished, thus giving rise to a certain rebound action which coupled with their acquired onward motion, and with the centrifugal impulse they receive by frictional contact with the lower atmosphere, constitutes them a surface stream flowing from the polar to the equatorial regions, and thence out into space. (Lest I should be misunderstood, allow me to add that I do not look upon centrifugal action as sufficing in any way to overcome solar gravitation.) Astronomers are in the habit of regarding each spheroid possessed of an atmosphere as rotating in vacuous space; under such circumstances the atmosphere must partake of the rotatory motion of the solid spheroid, and after having attained an increased depth at the equator, will assume a state of static equilibrium unless disturbed by external influences. No such statical equilibrium is possible, however, if we assume the same spheroid with its atmosphere, surrounded by an ocean of indefinite dimensions, consisting of gaseous matter not partaking of the rotation of the spheroid, although subject to its attractive influence. Equal masses will under those conditions be equally attracted both in the polar and equatorial direction, and the continued disturbance of equilibrium by rotatory motion must result in continuous outflow. Nor need this outflow be accomplished entirely at the expense of rotatory motion of the spheroid because the inflowing polar current when once established, will only have to be changed in direction by frictional action in order to convert it into the outflowing current.

2. Regarding the second question, I assume that the atmosphere of each spheroid in space is precisely such as would result from its mass, and if this view is correct, the moon also must have an atmosphere, though of so attenuated a character as to be scarcely perceptible by means of optical instruments; for as Wollaston put it in his celebrated paper, read before the Royal Society in January 1822, "it would not be greater than that of our atmosphere is, where the earth attraction is equal to that of the moon at her surface, or about 5000 miles from the earth's surface." I am well aware that in assuming atmospheric air to be a perfectly elastic fluid, the atmospheric density would at a height of only 70 kilometers not exceed the 1-7000th part of atmospheric density, and would therefore at greater distances become inappreciable; but we have evidence to show that Boyle and Mariotte's law holds good only within comparatively narrow limits, and there is other evidence referred to in my paper in favour of the supposition that such gases as are contained in meteorites are diffused through space in appreciable amounts, or the meteorites could not for millions of years have retained these gases, notwithstanding the action of diffusion into empty space.

3. The amount of vapour that would condense upon the earth under the conditions here assumed, would depend upon its mean temperature on the one hand, and on the vapour-density of the stellar atmosphere surrounding it on the other. Assuming the density of the stellar atmosphere, which, while surrounding the earth does not partake of its rotatory motion to be 1·10,000th part of atmospheric density, and saturated with aqueous vapour, the point of condensation would be according to Regnault − 50° C., if the outer regions of our atmosphere should be at that temperature, and saturated with aqueous vapour, the two would be in diffusive balance; if they were at a lower temperature they would acquire, and if at a higher they would part with aqueous vapour to the surrounding medium.

4. It has long been held by astronomers that there are stars beyond our range of vision, which hypothesis would involve that of absorption of heat and light rays in stellar space; some rays are more easily absorbed than others; thus it appears to be the yellow rays which are most efficacious in the decomposition of carbonic acid and aqueous vapour in the vegetable cell. May not the same conditions prevail in space, and allow probably the rays of highest refrangibility to pass on to the greatest distance without being absorbed—I should say utilised—in doing chemical work?

C. WM. SIEMENS

12, Queen Anne's Gate, S.W., May 22

Porculia Salvania (Hodgson)

A MOST valuable and interesting addition has recently been made to the Zoological Society's collection in Regent's Park, of four—a male and three females—Pigmy Hogs (*Porculia salvania*, of Hodgson) from the Doars of Bhotan. The extreme rarity and difficulty of procuring this animal makes its presence here of the greatest interest, and these individuals will be examined eagerly, not only by naturalists, but by many Indian travellers, sportsmen, and others, who have heard of, but never had the opportunity of seeing the pigmy hog. My attention was directed to it many years ago by the late Mr. Blyth, then in Calcutta, who on my first expedition to the Nepaul Terai, in 1855, requested me to endeavour to obtain a specimen—as far as I remember, neither Blyth nor Jerdon had seen it living—Hodgson, who described and named it, had heard of its existence from the Nepalese or other denizens of the Terai, or neighbouring localities, long before he obtained a specimen. I was unable to procure one, though I made repeated attempts to do so, and enlisted many influential friends in the search, but without success; very few appeared to know even of its existence, whilst many seemed to regard it as mythical. Occasionally I met with natives who said they had heard of it, but I began to fear that it might be extinct. The four fine specimens now in the Gardens prove that such is not the case, and will furnish opportunity of supplementing Hodgson's description of the animal, which is to be found in the *Proceedings* of the Zoological Society, and in Jerdon's "Mammals of India."

These lively little pigs, weighing probably hardly as much as a hare, are most active and energetic; they resemble the ordinary pig in miniature, but probably may have some anatomical peculiarity which will interest naturalists as regards affinity with the Peccaries. The specific designation Salvania, is from the Sal (*Shorea Robusta*), as the pig is, I believe, found in that part of the Terai and along the sub-Himalayan tracts, where the Sâl tree abounds, and among the long grass in which the little creature hides itself. It is much to be hoped that they will breed, and thus enable other zoological collections to be supplied with specimens of a most rare and interesting species.

J. FAYRER

Pseudo-Glacial Phenomena

I BEG to call the attention of geologists to the following facts:—On the north-east coast of Australia, at the end of Trinity Bay, about lat. 17° S., there are steep ranges of granite abutting on the sea-margin. Every rainy season (December, January, and February) immense quantities of the granite

become loosened from the upper part of the mountains, and fall in extensive landslips down the sides. These landslips or rockslips are so numerous, that in fine weather they are most conspicuous objects on the sides of the hills, and look like dry water-courses. One of these rockslips I witnessed at Cape Grafton, from a distance of three miles. The noise was terrific, and the ground trembled as though from an earthquake. On examining the blocks of granite which had slipped to the bottom of the ravine, I found many of them with their sides grooved and scratched, and one fragment was as beautifully polished on one side as if it came from the hands of a lapidary, excepting, of course, the scratches and grooves. In the course of a few centuries, much of the range will be worn away, and its sides represented by an alluvial deposit mainly consisting of angular boulders of every size and shape, many of which will be polished, scratched, and grooved. There are very few geologists who would not call it a glacial drift, even now, were not the cause so evidently before them. Will this help to explain tho e so-called drifts, which, like this instance, are found far within the tropics? T. E. TENISON-WOODS

Union Club, Sydney, N.S.W., March 25

Variability of Number of Sepals, Petals, and Anthers in the Flowers of Myosurus minimus

IN my article on "Different Modes of Self-fertilisation where Visits of Insects are wanting" (NATURE, vol. x. p. 129), I gave a short account of the number of sepals, petals, and anthers in a hundred flowers of *Myosurus minimus* examined by myself. Some error must, however, have slipped into this account, the sum of the quoted flowers differing from a hundred. I have, therefore, lately repeated my examination and give here the results. In 200 flowers I now found 35 different proportions in the number of sepals, petals, and anthers. These were contained in—

	Flowers	Sepals	Petals	Anthers		Flowers	Sepals	Petals	Anthers
	1	4	3	4	*c*	2	5	5	3
	1	4	3	6	*c*	3	5	5	4
	6	5	2	3	*c*	7	5	5	5
	4	5	2	4	*c*	7	5	5	6
	1	5	2	5	*c*	4	5	5	7
	1	5	2	6	*c*	3	5	5	8
a	1	5	3	2		1	6	2	5
a	4	5	3	3		1	6	2	6
a	10	5	3	4		1	6	2	7
a	33	5	3	5		1	6	2	8
a	23	5	3	6		1	6	2	10
a	24	5	3	7		1	6	3	7
a	9	5	3	8		1	6	5	7
a	1	5	3	9		1	7	2	7
b	6	5	4	4		1	7	3	6
b	7	5	4	5					
b	16	5	4	6					
b	14	5	4	7					
b	2	5	4	8					
b	1	5	4	9					

In general, the number of sepals, petals, and anthers increases and decreases with the size of the flower, the 12 first quoted flowers being exceedingly dwarfish ones.

It should further be considered that in combination with a certain number of sepals and petals a certain number of anthers seems to be the normal one, and from this normal (maximum) number of anthers, as to be seen under *a*, *b*, *c*, the numbers of flowers on the two opposite sides are constantly decreasing.

Lippstadt, May 16 HERMANN MÜLLER

"A Dead Heat"

TELEGRAMS from Paris on Monday state that the "Prix du Jockey Club" had resulted in what is usually called a "dead heat." It is unnecessary for me to inform you, that there can be no such thing as a "dead heat." It is called so, I suppose, in consequence of a disagreement among the judges as to which horse first thrusts his nose beyond the winning-post. Are living judges any longer necessary to determine the results of a race? Five years ago I proposed to prove by indisputable evidence the winner of a trotting match which, in consequence of a dispute among the judges, had to be trotted over again. By means of a single thread stretched across the track, and invisible to either horses or their riders, twenty photographic cameras have been made to synchronously rec rd positions impossible for the eye to recognise. With the aid of photography, the astronomer, the pathologist, the chemist, and the anatomist are enabled to pursue the most complex investigations with absolute confidence in the truth it reveals; why should those interested in trials of speed not avail themselves of the same resources of science? I venture to predict, in the near future that no race of any importance will be undertaken without the assistance of photography to determine the winner of what might otherwise be a so-called "dead heat."

449, Strand, W.C., May 23 EDWARD MUYBRIDGE

Aurora Borealis

THE auroral display mentioned by your two correspondents was particularly brilliant at Oldham on the evening of the 14th inst., at 11.10. I observed at 11.15 one very fine streamer reaching quite to the Pole Star; it was of a ruddy hue, dull, and changing to purple. The horizon was cloudy, the cloud being fringed with white light, changing to rose colour. The constellation Cassiopea was at times covered with a mass of light, from whence the streamer arose, lighting up the whole of the northern sky. W. PULLINGER

Oldham

Bright Meteors

1882, May 16, 11h. om. G.M.T. Meteor many times brighter than Venus; green, then white; began of second magnitude, 5° above main cluster of *Coma*; passed $1\frac{1}{2}^\circ$ above *Iota* Urs. Maj., where it changed colour suddenly; ended, of second magnitude, 5° left of *Beta* Aurigæ. Duration 8 seconds, may have been 10. No streak. Observed from the University Observatory. A few minutes later another was seen describing very nearly the same path. G. L. TUPMAN

Oxford

Curious Formation of Ice

IN your issue of November 24, 1881 (vol. xxv. p. 78) Mr. J. F. Duthie described small wafer-like, rather funnel-shaped pieces of ice which he noticed in October, on the slopes of the Himalayas, and asked whether such forms of ice had been observed elsewhere.

On November 30, 1881, I observed, at a height of about 7000 feet, near the hill station of Chakrata, on the outer Himalayas, ice crystals which were grouped in bundles about one inch long and one inch in diameter. The bundles consisted of prisms up to a quarter of an inch d ameter, and looked at from the side the long parallel prismatic faces, and the short rectangular outlines of the ends of the prisms suggested rather the orthorhombic system of crystallisation. On looking straight at the end of the crystals, it was, however, seen that all the prisms were hexagonal, and that they ended in hollow hexagonal pyramids, thus bringing out clearly the hexagonal system of crystallisation to which ice belongs.

The hollow hexagonal pyramids showed further development in other portions of the hoar frost, and there seems very little doubt that what Mr. Duthie describes were accumulations of small crystals originally grouped in the shape of hexagonal hollow pyramids, but more or less expanded and rounded off.

I may here mention another interesting occurrence of crystals which I had the opportunity of noticing at the salt works in Cheshire. During slow evaporation of brine in a steam-heated reservoir, crystals of salt formed at the surface in the shape of hollow hexagonal pyramids. This is easily explained. Whilst the ordinary well-known hollow salt pyramids with square base form, by the gradual sinking and growth of a cubical nucleus which floats with one pair of faces horizontally placed, these exceptional hexagonal pyramides form from an original cube which floats on the water with a solid angle as its lowest point. The six lateral edges are the beginning of the hexagonal pyramids. H. WARTH

Dehra Dun, N.W. Provinces, East India, April

The Existence of a Voice in Lizards

THE following may perhaps be of some interest in connection with the letter of Prof. Th. Eimer (vol. xxvi. p. 29). One evening as I sat in the verandah of my house in Madras, my attention was called by a peculiar cry, and on looking

up I saw that it was being made by a small lizard, apparently in a state of great terror at a snake which was uncoiling itself from the rafters close to it. I cannot say what the species of the lizard was, but it was one of those so abundant on the walls of Indian houses; it was one of two that appeared every evening, when the lamp was brought out into the verandah, and feasted on the moths attracted by the light. C. MICHIE SMITH

20, Duke Street, Edinburgh, May 5

THE ECLIPSE EXPEDITION

THE *Daily News* of Tuesday publishes the following communication from its special correspondent with the English Eclipse Expedition:—

On the Nile, between Siout and Sohag, May 6

The astronomers have now nearly reached their destination if all goes well, but it is by no means certain that everything will, for the Nile has never been known to be so low, and we have already been aground many times. To-morrow morning will see them and their instruments landed after their last water journey. The arrangements made by the Egyptian Government and by His Highness the Khedive himself, who takes the liveliest personal interest in the work, have been simply perfect. Everything seems to have been foreseen, every possible cause of delay obviated, and everything that could conduce in any way either to the comfort of the observers or the success of the observations provided. One officer of the Egyptian Government or another has been in constant attendance upon the expedition since they landed at Suez, and any indication of a desire that a certain course of action should be taken has been at once attended to. There is no shutting one's eyes to the fact, that there are many men occupying high positions in this wonderful country, through which we are now journeying, who take the keenest interest in scientific progress, and who are more than anxious, that Egypt should take her place among the more highly civilised nations, among whom science is cultivated to a greater or less extent; and there can be little doubt, that the efforts now being made to educate the people will in time bear more fruit of this soil. It is quite *apropos* to this train of thought to mention, that the fact that the admirably equipped observatory of Cairo will count for very little among the proposed observations is keenly felt. It must not, however, be imagined that because the metropolitan observatory counts for so little, Egypt possesses no astronomers. I mentioned in a former letter that it was hoped that Ismatt Effendi, a member of the Khedive's household, might be attached to the expedition. When the expedition reached Suez and the *Kaisar-i-Hind* steamed into the harbour, it was easy to see that something unusual was going on there. The Khedive had not only sent Ismatt Effendi to receive the expedition, but had sent orders to the Governor of Suez to welcome it in his name. Nor was this all. A special train had been provided for the instruments and observers, and every precaution taken for safe handling and safe custody of the former. In the morning the Governor of Suez speeded the parting guests, who were accompanied by M. Ismatt, and this gentleman soon showed how much he had profited by the long training he has received in the observatories of Paris and Washington, and it was soon acknowledged that in him the expedition not only found a most useful and agreeable companion, but a collaborator of the highest value.

On arrival at Cairo the party found General Stone, chief of the staff, accompanied by Moktah Bey, on the platform to welcome them. Of the former, it may be said that his influence for good on the higher education in Egypt and on the officers who have served under him is freely acknowledged. He has lost none of the enthusiastic love of truth for its own sake, and of science for the sake of the world, which characterises so many of the best of his countrymen; and many of his remarks touching his conception of the duty incumbent on the Government of Egypt, in aiding a work of international aim, strongly reminded me of General Sheridan's thoughts and words when he received at Washington one of the English observers of the eclipse of 1878. Moktah Bey, who has been detailed to accompany the expedition, is an officer who has greatly distinguished himself by his travels in the Soudan. He is not only an admirable administrator, but a capital linguist, while his love of work in the Soudan and in Upper Egypt in triangulation, determining latitudes and longitudes, and establishing, or endeavouring to establish, meteorological observatories, nileometers, and the like, render him also a valuable scientific ally.

General Stone, on the arrival of the instruments at Cairo, cut a Gordian knot by at once ordering the car containing them to be ferried across the Nile. There is no railway bridge over the Nile at Cairo, so that considerable time was saved and risk avoided by this measure. In fact it may be said that not only were the cases containing the instruments untouched from Suez to Siout, but that they were actually sealed up all this time. There was not much time to give to the strange sights and old memories of Cairo; even the Pyramids have remained unvisited so far, for after resting one night and spending part of the next day in official visits, the party was off again yesterday and travelled during the night down to Siout, the most southerly railway station in Egypt, and about 70 miles north of the point where the eclipse line crosses the Nile. At Siout, whither extra camels and porters had been summoned by telegraph, the sight at sunrise this morning was strange beyond all description, or at all events beyond the descriptive powers of your present correspondent. The telescopes to be used on the present occasion are of very great weight, and although they have been divided into as many separate pieces as possible, some of the cases are still very heavy, taxing the powers both of men and camels to the utmost. The camels, which were made to kneel down so soon as the cases had been got out of the railway car, groaned as they rose with such an unaccustomed weight; and giant Arabs, good-natured sons of Hercules, did the rest at the boat side. But here again a special arrangement was necessary. The Nile is so low and the steamer was already so crowded that the instruments were placed in a special boat taken in tow by the steamer. Events have shown that this precaution was by no means an improper one, for during the last hour and a half we have been firmly aground, and it does not seem as if all the shouting of the motley crew, or any manœuvring of the engine is going to get us off again. While these attempts, which seem born of confusion, are wearing themselves out, it will be as well to say a word about the final arrangements, so far as they are known to us, before we actually arrive at the station.

There are three expeditions in Egypt for observing the eclipse—a French, an English, and an Italian one. As there is so little choice of station, his Highness the Khedive has sent forward a steamer to Sohag, the point at which the eclipse track crosses the Nile, and has invited the various expeditions to use this as a *point d'appui* and floating hotel wherever their actual place of observation may be. Prof. Tacchini forms part of the Italian party, and MM. Thollon and Trépied of the French one. The latter have gone on ahead, and it is thought that their instruments are already up and adjusted, while the Italian party follows us on Monday. It is believed that the work of both of these parties will be chiefly spectroscopic. As the exposure of Capt. Abney's plates forms an important part of the English plans, it is intended to take special precaution for securing the photographic rooms and tent from dirt. With the same object in view covers have been prepared for the telescopes, which closely fit them and can be kept, it is fondly hoped, sand-tight. This brings me to say a word about the khamseen. The season this year

in Egypt has been extraordinary. There has been much rain and very little khamseen, and now there is a brisk north wind blowing, which generally follows the dust wind. Hence many weather-wise people say that the khamseen is over; if so, of course, all the better. On the other hand, to-night from our sandbank we have witnessed a sunset rendered transcendentally beautiful by clouds over fully one-half of the sky. It is possible therefore that if the present weather continues, the sky will not be quite so free from vapour as it is generally in Upper Egypt. To avoid the khamseen, General Stone, who has had the region reconnoitred, has suggested to the English party to occupy an eminence to the north-west of Akmin, a village a little higher up the river than Sohag.

Near Sohag, Sunday

I had got so far at 11 last night, when the time came for closing the mail bag, although we were fast aground, and apparently with less chance than ever of getting off. There were two mail bags, however, made up after all, for the service is so interfered with higher up the Nile that I am still in time on Sunday evening to send a letter which ought to catch the next Brindisi mail, though whether it does or nor is very doubtful, for we have been aground again twice to-day.

So, as we have at last arrived at our station, I will endeavour to give an idea of the proposed arrangements. In the first place, we have found the steamer on which the various parties are to live as the guests of the Khedive moored close to the shore, at a point where it trends north and south, or very nearly so, about half a mile below Sohag. This position, which has been selected by the French party—the first to arrive—is a very admirable one for two reasons. First, the constant wind during the last week has been from the north, and by keeping a staff of people watering the foreshore of the Nile, all dust is obviated. To the north of the place of observation trees, and what looks like grass from a distance, grow close to the margin of the river; so that the dust can only be of nearly local origin; while a long stretch of sandbank to the north, running east and west, is far enough away to deposit its sand in the Nile before it can reach us.

Secondly, the khamseen, if khamseen there is to be, will have to travel a mile and a half along the Nile before it can enter the observatories; and it is thought this amount of water surface will have an important effect in reducing the amount of sand in the air, even in its case also. *Nous verrons.* These considerations have induced the English party to take up ground close to the boat and their French confrères. The hills which look so tempting in the mass are simply impossible as places of observation. With the means at command here it would take a week to get the instruments up, much more in position; while at Akmim, which is only two or three miles away, there does not seem to be any spot more favourable, taking everything into consideration, than the one here.

At five o'clock to-morrow, then, the work will begin, and the next week will be a busy one, for in spite of the fresh breeze and the clouds—for there are very distinct clouds to-day--work on the sand becomes very oppressive in the middle of the day, and there are heavy weights to move, which the observers must move themselves. The scene from the ship is already interesting. To the north two tents and various shelters, to the south one tent. These will increase to six to-morrow. Here and there groups, looking down the bank, stealthily from between the trees. There is a pretty thick grove of acacia trees, which shelter us somewhat from the rays of the setting sun, still fierce in this latitude. Here and there, skirting the grove, a sentinel with fixed bayonet keeping guard. At the extreme south, tents for the military, and a long line of piled arms.

Across the water the scene is novel and beautiful in the extreme. The main Nile, in which the boat is anchored, is here about half a mile wide, but there is an island about two miles long, and a wide stretch of water beyond that. This island forms, with the river, the foreground of the landscape. With an opera-glass we can see the Fellaheen cultivating the ground almost to the water's edge in places, and looking after their crops of maize or their flocks of goats. Here comes a veiled Rachel to the sacred river to fetch water for a house in an indistinct flat-topped village, sheltered in a large group of beautiful palm trees. The arm of the river beyond the island we cannot see, but a background is not lacking. A long line of mountains, we may almost call them, full of geological tracery, are now, as I write, almost blood-red in the light of the setting sun, and are surmounted by that grey purple one always sees to such advantage in Eastern lands—both grey and purple haze in a few hours to give way to the silver dawning of the moon, now terribly dwindling in her visible surface, and reminding the astronomers of the coming seventy seconds in a most forcible manner.

The proceedings at the end of the first day on which the English and French parties found themselves together as guests of the Egyptian Government naturally included some toasts—that of his Highness the Khedive, proposed by M. Trépied, and that of the English and French nations, proposed by Moktah Bey, and responded to by Mr. Norman Lockyer. The arrangements on board are as perfect as those made at the various stations on shore, and one's national pride is a little touched at the idea of what the Government reception would be of a party of Egyptian astronomers coming to England to observe an eclipse of the sun.

ANEMOMETRICAL OBSERVATIONS ON BOARD SHIP[1]

IT is known that the determination of the velocity of the wind in the ocean has always been one of the desiderata of meteorological observations. Maury devoted much attention to this subject, and to determine, at least approximately, the velocity of trade-winds, he was compelled to work on a very unsafe basis—the velocity of ships during different parts of the year—and to put aside all observations made in accordance with the scale of Beaufort as unreliable. But it is obvious that the velocity of a ship depends on so many circumstances quite independent of the wind itself (such as the shape of the ship, the surface of its sails, the disposal of the cargo, and so on), that its velocity is but a very imperfect means of measuring the velocity of wind. Besides, the relation which exists between the force of the wind and the velocity of a ship, under different angles between the direction of both, is a new source of error, as this relation has not yet been established with accuracy, and can be established only by means of anemometric measurements. The necessity of trustworthy measurements of the velocity of wind at sea was so well understood in England that the Royal Society and the British Association established in 1859 two anemometers—one on the Bermuda Islands, and the other at Halifax. But it is known that the force of the wind is usually lessened on continents and islands.

Therefore it was absolutely necessary to make anemometrical observations on board ships, and a few attempts had already been made in this direction. Prof. Piazzi Smith invented an anemometer which might be established on board a ship, and which merited the highest eulogy from Maury, but Lieut. Domojirov does not know if any observations were made with it. Emil Bessel, during the Arctic expedition of the *Polaris*, made a series of observations with an anemometer on board his ship, but he does not explain, neither the methods of observation, nor the corrections he applied to his measurements. In

[1] A. Domojirov, in the *Izvestia* of the Russian Geographical Society, vol. xviii. 1882, fasc. 1.

1879 Col. Rykatchoff, of the St. Petersburg Central Physical Observatory, established, on board the *Nayezdnik*, an anemometer of his own construction, which was observed during the ocean cruise of the clipper; and the schooner *Nordenskjöld*, which unhappily was lost in 1879 at Yesso Island, had also an anemometer, which was taken afterwards on board the Russian clipper *Djighit* by M. Domojirov. The observations on board of the *Djighit* were carried on with this anemometer (of Casella) put in such an apparatus (like that of the lamps on board of ships), as always maintained it in a vertical position, even during the heaviest seas, when the ship oscillated for 30° on one side, and 35° on the other. The apparatus was put on a 16-foot-long pole, which was pushed out for each observation on the wind-side of the ship, from the boat, and thus exposed to the full force of the wind for ten minutes. The height of the instrument above the sea was 26 feet. The direction of wind was determinated by means of a vane, and its true direction computed from the apparent one, on the principle of the parallelogram of forces, by taking into account the velocity of the ship. When the angle between the direction of the wind and the direction in which the ship goes is known, as well as the seeming velocity of wind (measured by the anemometer), and the velocity of the ship, the true velocity of wind is easily determined by means of simple computations, or of the tables published for that purpose by M. Rykatchoff (*Russian Marine Review*, February, 1880). A series of experiments having been made for ascertaining in how far the calculated figures agree with the true ones, M. Domojirov arrives at the conclusion that these figures are quite reliable; determination having been made during the progress of the ship, she was immediately stopped, and the determination made anew, both results always being quite identical. But the measurements from the side-boat are very tedious and even dangerous during heavy seas, and each observation occupies no less than three men for about twenty minutes. Therefore M. Domojirov proposes to apply electricity to register the rate of the anemometer.

The observations on board the *Djighit* were made five and six times per day from March 23 to May 30, and the complete results, with all elements for calculations, are published in the papers of M. Domojirov. The north-eastern trade-wind, observed on the passage from Japan to the Sandwich Islands and back, had a very regular force of from 5 to 9 metres per second; the south-eastern trade-wind experienced on the passage from the Sunda Islands to the Seyschels, had a velocity of 4 to 9 metres per second, and the south-western winds on the passage from Port Victoria to Aden, had velocities from 12 to 15 metres per second.

It would hardly be necessary to insist on the importance of such observations for meteorology, as well as for practical purposes, and we hope that soon the still prevailing prejudices as to the possibility of anemometrical observations on board ships having disappeared, and more convenient methods of observation having been devised, the anemometer will become on board ships as necessary an instrument as the log and barometer.

P. K.

INSTANTANEOUS PHOTOGRAPHY OF BIRDS IN FLIGHT

PROF. E. J. MAREY has lately published in the pages of our contemporary, *La Nature*, an article on a "photographic gun," the illustrations to which, with a somewhat shortened account of the process, we are enabled, through the courtesy of the editor of *La Nature*, to present to our readers. M. Marey's researches on animal locomotion are well known; his experiments carried on by the graphic method were productive of most valuable results, and they corrected and explained many debated points in animal mechanics; but having seen some of the results obtained by Mr. Muybridge, at San Francisco, with photographic pictures taken during an exposure of the 1-500th of a second, he was very desirous to have the same process adapted, so as to admit of its being applied to the taking of birds flying. In September, 1881, on a visit of Mr. Muybridge to Paris, he brought with him some photographs of birds taken on the wing, but these unlike the invaluable series taken by the same gentleman of horses and men, were not the representation of a series of continuous attitudes, but rather represented the bird in the position it happened to be in at a moment of time; whereas, to explain the fall and rise of the wings and the positions of

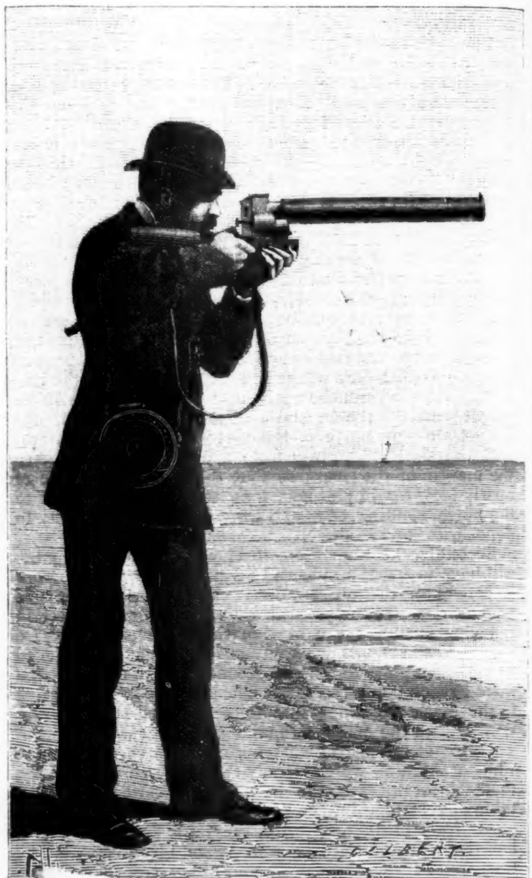

Fig. 1.—The Photographic Gun.

the body, it was, above all things, important to have a series of rapid photographs taken of the same bird over a period during which the whole mechanism was in action, so as to allow of the movements to be afterwards studied at leisure. After deliberating over this subject during the last winter, at last the idea of a photographic gun occurred to him; but the immense quickness with which the movements should succeed one another, in order to bring a series of sensitive surfaces across the lens, at first presented great difficulties in the constructing of the machine. It was necessary to have images taken successively ten or twelve times in one second, in order to

succeed in obtaining the various positions of the wings of a bird at each revolution. As the result of a good deal of thought and labour, an apparatus was constructed about the size of a sporting-piece (Fig· 1), which would take twelve images, in one second, of an object on which the piece was continuously sighted. The time of exposure of each image was about 1-720th of a second.

The barrel of the gun is a tube containing a photo-

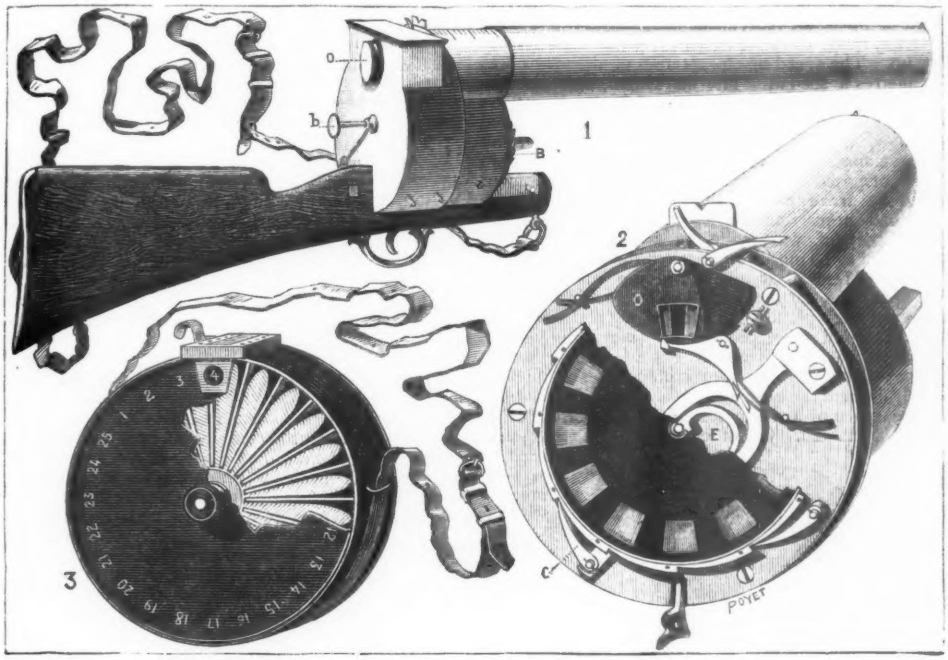

FIG. 2.—Mechanism of Gun. 1, General view; 2, Windowed disc; 3, Box with 25 sensitive plates.

graphic object-glass. Behind this, and solidly mounted on the butt, is a long cylindrical breach, containing clock-

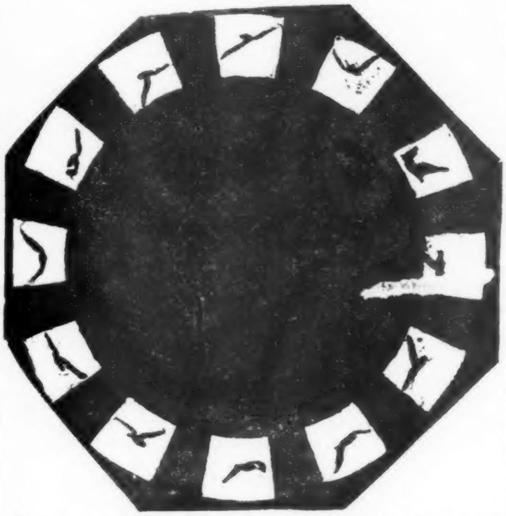

FIG. 3.—Sea-gull, flying; heliograph of twelve plates obtained by the process.

work, of which the exterior of the barrel is seen at B, Fig. 2, No. 1. On pressing the trigger the clock-work is set in action, giving to the different parts of the instrument the necessary motion. A central axis, which makes twelve revolutions per second, governs all the pieces of the apparatus. Of these one is a disk of metal pierced with fine openings, which acts as a diaphragm, and only allows the image of the object to be presented twelve times in a second, and each time only

FIG. 4.—Sea-gull; beginning of depression of wing.

for 1-720th of a second. Behind this, and revolving on the same axis, is another disk carrying twelve windows, behind which are the sensitised plates. This windowed disk revolves in such a way as to be slightly arrested in its course twelve times in a second, when the open windows are exposed to the light. The teeth to arrest its movements are seen at C, Fig. 2, No. 2, while the excentric at E, Fig. 2, No. 2, keeps up the regularity of this

movement. A cover over all keeps the light out from the rear of the instrument. It will be noted that during the instant of exposure, the sensitised surface is steady, and when the exposure is over, it is at once passed away. Pressure on the button (*b*, Fig. 2, 1) sets the machine in motion. Before applying this instrument to the study of the flight of birds, an experiment was made with it on a black arrow, made to rotate against a white back-ground well lit up. The speed of the rotation of the arrow was about 5 metres a second. The marksman, at a distance of 10 metres sighted on the centre of the target, on which the eye could perceive nothing save a confused grey shadow, so quick was the rotation of the arrow: but on the development being completed, twelve images were to be seen, disposed in a circular manner, and each showed not only the arrow, but its shadow, as sharp as if the original had been immovable. Another experiment, equally successful, was made on a pendulum beating seconds. For to be more certain as to the duration of the exposure, M. Marey next adapted to the gun a chronographic apparatus, so that the time intervening between the taking of each picture could be with precision ascertained. After all these preliminary essays, the photography of animals in movement was attempted; and in Fig. 3 there will be seen the photographic representations of a sea-gull, in which the twelve successive attitudes assumed during the space of a single second by this bird during flight are ascertained. On other occasions other successful photographic series of a sea-gull in flight were taken when the bird was seen less in profile. The sea-gull gave exactly three strokes of its wing each second, so in the twelve photographs of each stroke four succeeding stages were reproduced. The wings at first elevated to their greatest, then commence to be lowered; then in the following image they are seen at the lowest point of their course; and in the fourth image are again on the rise. In enlarging these images, figures seen from a good distance were obtainable, but the sharpness of the enlargements left a good deal to be desired; for the negatives were somewhat granular, no doubt owing to some slight fault in the photographic process. The reproduction of these images by the heliographic process gives excellent silhouettes (as seen in Figs. 4 and 5); the originals, when examined under the microscope, showed even the wing-feathers distinctly.

FIG. 5.—Sea-gull; end of depression of wing.

On comparing the indications thus given by the photographic process with those already attained by the graphic process, a confirmation of most of the principal points obtained by the latter were obtained; but otherwise so far, the latter did not seem to add much to our knowledge of the mechanism of flying. However, ere deciding that this is so, numerous observations on different birds flying and in different conditions of flight, during calms and storms, and with and against the wind, must be taken. Attempts were also made to photograph the bat, but its small size, its flight during the dusk, and its capricious method of flying made it a difficult subject; but some of the experiments revealed interesting results. The angle of oscillation of its wings is very extended, especially from below, when the two wings form two vertical planes sensibly parallel.

These extremely interesting researches of M. Marey are only, as it were, in their infancy; he intends pursuing them much further, and his results will be looked for with great interest by all those who study the subject of animal motion.

DR. FRITZ MÜLLER ON SOME DIFFICULT CASES OF MIMICRY[1]

IN his original explanation of the cause of mimicry, Mr. Bates referred to the occurrence of many cases in which species of different genera of Heliconidæ resemble each other quite as closely as the mimicking Leptalides and Papilios resemble species of Ithomia and other Heliconoid butterflies. In these cases both the imitating and the imitated species are protected by distastefulness, and it was not therefore clear how the one could derive any benefit by resembling the other. Accordingly, Mr. Bates did not consider these to be true cases of mimicry, but to be due, either to identical parallel variations of externally similar form, or "to the similar adaptation of all to the same local, probably inorganic, conditions."

Examples of this close resemblance of species of different genera of protected groups have now become very numerous, and they often extend to three or more distinct genera, some species of which imitate each other in most parts of tropical America, each changing in a corresponding manner as we pass from one district to another.

In my Address to the Biological Section of the British Association at Glasgow, in 1876 (reprinted in "Tropical Nature"), I connected these cases with a number of others in which peculiarities of colour or of form appear together in several groups not closely allied, but always among those inhabiting the same locality and as frequently among unprotected (that is, eatable) as among protected groups of butterflies; and I concluded, generally, as Mr. Bates had done, that these curious phenomena were due to "unknown local causes."

Thus the matter rested, till, in 1879, Dr. Fritz Müller published in *Kosmos* a paper on "Ituna and Thyridia; a remarkable case of Mimicry in Butterflies"; and in 1881 a second paper, entitled "Remarkable cases of acquired resemblance among Butterflies," in which he gives a solution of the problem as really a case of mimicry. The first of these papers was translated by Mr. R. Meldola, and communicated to the Entomological Society of London in May, 1879, and the same gentleman has kindly furnished me with a translation of the second paper (the title of which is given below), which discusses the whole question in great detail, and devotes much space to a criticism of my suggested "unknown local causes" as a sufficient explanation of the phenomena. I may at once say that I admit this criticism to be sound; and that Dr. F. Müller's theory appears to me to afford a clue (with some slight modifications) to most of the cases of close individual resemblance of not-nearly-related species of butterflies yet observed. I therefore wish to state, as briefly as possible, the exact nature of the explanation now afforded us, and this is the more necessary because Dr. Müller's theory did not receive much support when brought before the Entomological Society, nor did it then satisfy Mr. Bates, the discoverer of the true meaning and importance of the phenomena of mimicry as interpreted by the doctrine of Natural Selection.

The explanation depends on the assumption, that some at least, if not all, young insectivorous birds learn by experience that the Heliconoid butterflies are distasteful, and in so doing sacrifice a certain number of individuals

[1] "Bemerkenswerthe Fälle erworbener Aehnlichkeit bei Schmetterlingen." Von Fritz Müller. (Separat-Abdruck aus "Kosmos," V. Jahrgang, 1881.)

of each distinct species. But if two species, both equally distasteful, closely resemble each other, then the number of individuals sacrificed is divided between them in the proportion of the square of their respective numbers; so that if one species (a) is twice as numerous as the other (b), then b will only lose one-fourth as many individuals as it would do if it were quite unlike a; and if it is only one tenth as numerous then it will benefit in the proportion of 100 to 1. It is an undoubted fact that the species of protected butterflies, like those of other groups, differ greatly in abundance of individuals, some being very rare while others are among the commonest of all butterflies. The proportion of 100 to 1, therefore, is far below the amount of benefit an uncommon species might derive by resembling a common one. The benefit to be derived is thus clear, if the protected species are subject to the danger of attacks by young birds before they learn that such species are uneatable. I agree with Dr. Müller that they are exposed to this danger; and when we consider the great number and variety of insectivorous birds in South America the danger must be considerable, and quite sufficient to render it important for a numerically weak species to reduce it to a minimum, although to a species abounding in individuals it may be of little importance. It has been suggested that young birds have an hereditary instinct enabling them to distinguish uneatable butterflies antecedent to experience; but this seems in the highest degree improbable. It has no doubt been shown by Mr. Darwin that monkeys in captivity have a dread of snakes, and Mr. Jenner Weir believes that birds have an instinctive knowledge of uneatable caterpillars. But even admitting that in these two cases there is an instinctive hereditary aversion, it does not follow that the same will occur with regard to protected butterflies. Snakes form one well-marked group, and it is not alleged that monkeys distinguish between poisonous and harmless snakes; and caterpillars can also be readily divided into the two classes of edible and inedible by their green or brown (protective) colours on the one hand, and their gaudy or conspicuous colouration or hairy bodies on the other. But the protected butterflies have no such general mark of inedibility. Their colours and forms vary greatly, and cannot as a group be readily differentiated from those of other butterflies; and it is not to be accepted without actual proof that a young bird knows instinctively every Heliconoid or Danacoid butterfly in its district, as well as the protected Papilios and moths, almost infinitely varied as they are in colour and marking, among the equally numerous and equally varied butterflies of other groups It therefore seems clear to me that we have here a *vera causa* for the acquisition of true protective mimicry by the less abundant species of inedible butterflies.

There is however yet another cause which may have led to mimicry in these cases, and one which does not appear to have been discussed by Dr. Müller. The fact that the majority of butterflies are edible and are actually eaten by birds and other insectivorous creatures, while a considerable minority are distasteful and are thus protected, renders it pretty certain, *a priori*, that there exist many degrees of distastefulness. Certain species appear to be rejected by all insectivorous creatures, while some, though not eaten by birds, may be devoured by lizards, dragon-flies, or spiders. Some, too, may be eaten by some birds and rejected by others, and no ornithologist will think it strange or improbable that a trogon should have somewhat different tastes from a tyrant-shrike or a swallow. Again, in some species the distastefulness may extend to all the stages of egg, larva, pupa, and perfect insect, while in others it may be confined to one or more of these stages; or special dangers may exist for one species which are absent in the case of another. But it is evident, that, if these differences exist, it will be advantageous for the less protected to mimic the more completely protected species, and the fact of the affinity between the different genera, with perhaps some tendency to revert to a common style of colouration or marking, will afford facilities for the development of this class of mimicry even greater than occur in the case of the distinct and often remote families of completely unprotected butterflies. We need not, therefore, be surprised to find whole series of species of distinct genera of Heliconoid butterflies apparently mimicking each other; for such mimicry is antecedently probable on account of the greater need of protection of some of these species than others, arising either from some species being less distasteful to certain enemies, or less numerous, and therefore likely to suffer to a serious extent by the attacks of inexperienced birds. When these two conditions are combined, as they often would be, we have everything necessary for the production of mimicry.

The explanation now given, so far as it refers to the various degrees of protection, may be extended to explain those cases in which various groups of Nymphalidæ or other families appear to mimic each other; such as Catagramma, Callithea, and Agrias in one series, and Apatura with Heterochroa in another. In my "Tropical Nature" (p. 257) I have remarked—"Here, again, neither genus is protected, and the similarity must be due to unknown local causes"; but this is more than we know, and I now think that some of these groups—perhaps Catagramma and Heterochroa—are partially protected, and the advantage of sharing in this partial protection has led species of altogether unprotected and much persecuted groups to gain some protection by mimicking them, whenever their general form, habits, and style of coloration offered a suitable groundwork for variation to act upon.

If these views are correct we shall have the satisfaction of knowing that all cases of mimicry are explicable by one general principle; and it seems strange to me now that I should not have seen how readily the principle is applicable to these abnormal cases. The merit of the discovery is however wholly due to Dr. Fritz Müller; and it is to be hoped that he will complete his work by obtaining, if possible, evidence of its correctness. The chief thing required is an experimental proof of various degrees of inedibility in butterflies, during the different stages of their life-history; and also some observations as to the comparative abundance of the species of protected butterflies which mimic each other. If to this can be added the proof that such groups as Catagramma, which seem to be the objects of mimicry, are partially protected by inedibility, the chief remaining difficulty in the application of the theory of natural selection to all known cases of protective imitation will have been cleared up.

ALFRED R. WALLACE

NOTES

IN reference to the Darwin Memorial, to which we referred last week, the honorary secretaries have issued a circular asking for contributions to the fund. In this memorial it is stated that though the works of Charles Darwin are his best and most enduring memorial, it is felt by his many friends and admirers that these should not be the only one. They are desirous of handing down to posterity the likeness of a man who has done so much for the advancement of natural knowledge. They wish also to establish a fund associated with his name, the proceeds of which will be devoted to the furtherance of biological science A committee has accordingly been formed, of which Mr. T. G. Bonney and Mr. P. Edward Dove are the hon. secretaries. The committee is one of the highest influence, comprising the leading foreign ministers, the two Archbishops, and the best-known names in all ranks and professions.

JOHANN CARL FRIEDRICH ZÖLLNER, whose death we recently announced, was born at Berlin on November 8, 1834.

His first publication of importance was entitled "Grundzüge einer allgemeinen Photometrie des Himmels" (1861), and in 1865 followed the work, which must be considered his principal one, "Photometrische Untersuchungen mit besonderer Rücksicht auf die physische Beschaffenheit der Himmelskörper" (Leipzig, Engelmann). The photometer constructed by Zöllner compares the light of the celestial object observed with an artificial star produced by a constant source of light (the flame of a paraffin lamp kept at a constant height), which can be varied at will by turning two of the three Nicols through which the light from the flame has to pass. A crystal plate allows of variation of colour. With this instrument Zöllner observed both the moon, the principal planets, and the fixed stars, and the last chapter of his "Untersuchungen" contains an interesting attempt to explain all the various phenomena of variable stars, sun-spots, &c., by the gradual cooling of all the celestial bodies. In several later papers in the *Proceedings* (*Berichte*) of the Saxon Society of Sciences these ideas are further developed, particularly with respect to the nature and periodicity of sun-spots. Without knowing it Zöllner here followed in the footsteps of Buffon. Zöllner next directed his attention to spectrum analysis, and on February 6, 1869, he read a paper before the Saxon Society on a method of observing the solar prominences in full sunlight; but Zöllner did not obtain a suitable instrument for carrying out his idea till some months later. His "Reversionsspektroskop", which produces two spectra side by side and in opposite directions, has been utilised by himself and others for determining the rate of rotation of the sun by the shifting of the spectral lines at the opposite limbs. Most of his spectroscopic researches relate, however, to the solar prominences. In 1871 Zöllner published a paper on the nature of comets in the Leipzig *Berichte*, and the following year he republished this paper together with two papers by Olbers and Bessel, and a number of chapters "On the Theory of Comprehension" in his well-known book, "Ueber die Natur der Cometen," which excited much comment at the time, and made him many enemies. About the same time a new chair of "Physikalische Astronomie" was founded at the Leipzig University, to which Zöllner (who had for some years been extraordinary professor) was appointed. His astrophysical activity was at that time at its height, but soon after he turned his attention to spiritualism, which seems to have absorbed all his energy of late years.

IT is proposed to hold a meeting of the subscribers to the memorial to the late Prof. Rolleston on Thursday, June 1, at 3 p.m., in the Library of the Royal College of Physicians, for the purpose of determining the form that it shall take.

MR. J. L. E. DREYER, assistant at Dunsink Observatory, has been appointed to succeed the late Dr. T. Romney Robinson as director of the Armagh Observatory.

PROF. HAECKEL has returned to Jena from his voyage to Ceylon.

THERE have been great rejoicings at Lucerne this week in connection with the opening of the St. Gothard Tunnel.

WITH reference to the communication which we published last week, from Mr. T. F. A. Brown, on the cuckoo singing at night, we have received letters from several correspondents, detailing observations similar to those of Mr. Brown. It was not a previously unknown fact that cuckoos call at night, but the fact is probably not so familiar as it might be.

M. JANSSEN took magnificent photographs of the recent eclipse at the Meudon Observatory, Paris, where his revolver was set into operation to determine the first and the last contact. He also took two series of photographs 90 cent. diameter, one negative, and the other positive by direct exposure, with two large refractors. This is the first time that the whole photographic power of the Meudon Observatory has been set into operation. In the sitting of May 22 of the Academy of Sciences, M. Janssen presented the photographs of the last contact obtained with his revolver during the eclipse, on Daguerreotype plates. He stated that the inspection of the several images proved the contact to have taken place at a later time than that calculated. He presented also a large image, 90 cent. in diameter, obtained with his large refractor, and stated that he was unable to detect any difference in the immediate vicinity of the moon in the representation of faculæ and the minute details of the sun's structure. He considered the fact to be opposed to the existence of a sensible lunar atmosphere, as inferred from the spectroscopic observations of the French astronomers in Egypt.

ON May 17, M. de Mahy, the French Minister of Agriculture, presided at the laying of the first stone of the Observatory of Ventoux, at an elevation of 1912 metres above the level of the sea. M. Naguet, deputy to the French Lower House and Professor of Chemistry, delivered an eloquent address on the opportunity of establishing mountain observatories, as inaugurated by Leverrier on the top of Puy-de-Dôme (1465 metres), and practised at the Pic du-Midi (2877).

A LOCAL observatory has been established at Besançon, in order to determine the exact time by astronomical observations. This city is considered as the headquarters of French watch and clock making.

MR. C. L. WRAGGE, F.M.S., has just established a meteorological and climatological station at the Sanitary Depôt, Stafford. Mr. J. B. M'Callum, the borough surveyor, and his brother, Mr. T. M'Callum, have kindly undertaken to observe for Mr. Wragge every morning at nine o'clock (local time). The instruments are all standards, verified at Kew. The elements of observation consist of air and earth temperature, moisture, rainfall, direction and force of wind, kind and amount of cloud, hydrometers, and probabiy ozone.

GRIESBACH of Gera (Reuss, Germany) announces the issue of Frisch's edition of Kepler's Works (eight vols.) at about one-half the original price.

THE June number of the *American Naturalist* will contain a biographical notice of the late Mr. Darwin by Dr. Packard, and the articles will be devoted almost exclusively to the subject of Evolution.

M. W. DE FONVIELLE writes to us as follows :—"I am in a position to send a few interesting particulars of an aëronautical ascent which was made on May 18 by M. Eloy, in compliance with the programme alluded to in last week's NATURE. The ascent was made on the 11th day of a well-defined period during which the prevailing wind was almost without intermission a strong north-easterly breeze which has been detrimental to agriculture. The sky was clear deep blue, and the air cold and dry. A large number of dense small cumuli, dark, well defined, with round edges, were seen carried by with the wind almost without intermission, except during the eclipse, when the weather was magnificent. This period having terminated only on the 20h by a total change of wind, the observations taken may be considered as giving a fair idea of the atmospheric conditions which prevailed during so many days. These clouds were floating at an altitude of more than 2000 metres, and very cold, the thermometer having descended abruptly to $-4°$ and $-6°$ centigrades. When crossing this cloud, the aërial travellers perceived no isolated flakes of snow, but the air seemed illuminated by sudden lights, as if rays travelling from the sun had been reflected by minute icy particles. The balloon having ascended to the upper surface of the clouds, and travelled during more than an hour out of view of the land, the aëronauts were unable to perceive the

aureole round the shade of the balloon, which remained visible during the whole of the excursion on the upper face of the clouds. I explain this circumstance by the fact that the cloud was formed by solid water and that the aureole was less easily detected than when it is formed of vapour, being less brilliant, the same relation between these two phenomena existing for luminosity as between halos and rainbows. The aëronauts having remained at an altitude of two to three hundred metres from the clouds, were unable to perceive the coloured rings which were visible to me and M. Brissonet, navigating only at a few metres above similar legions of icy particles. It may have also occurred that our friends were blinded by the light from the sun, which at four o'clock was very powerful, and so detrimental to their eyes, that before entering the clouds they were unable to look fixedly at the earth to ascertain their path. It is the first time that I have heard of aëronauts having experienced the want of coloured spectacles to inspect our planet.

THE new Eddystone Lighthouse, which replaces Smeaton's famous work, built 120 years ago, was opened by the Duke of Edinburgh last Thursday.

UNDER date of Constantinople, May 17, an earthquake is reported to have occurred in the island of Karpathos.

THE first number of a small publication bearing the title of *Studies in Microscopical Science*, and edited by the well-known preparer of microscopic objects, Mr. Arthur C. Cole, F.R.M.S., "assisted by several eminent specialists," has just been published. It consists of a description and lithographed figure of a microscopical slide, which is issued, along with the description, to subscribers. The subject of this first number is yellow fibro-cartilage, and the preparation on the slide is a longitudinal vertical section of the pinna of the cow's ear. It is double stained in logwood and eosin, and is a well-mounted and highly-finished object. The plate is fairly good, though perhaps a little wanting in softness; it represents the section under a magnifying power of 333 diameters. The eight pages of descriptive letterpress contain—1. The name of the object and its etymology. 2. A very good description of the preparation under different powers, and of yellow fibro-cartilage in general, after the action of various reagents; also a few remarks upon its physiology. 3. An account of the different methods of preparation which may be employed, with their respective advantages and drawbacks; and lastly what seems a very complete bibliography of the subject. Altogether this first number has been well carried out, and promises well for the rest of the series. There is no doubt that *if the subjects are judiciously chosen*, this periodical will be a success, as it ought to be of great use to students and amateur workers in science. It is a pity that no list of the proposed subjects is given. It would be a decided advantage to know what the series for the present year will probably be, but beyond the fact that twenty-six histological will alternate with eighteen botanical, and eight petrological preparations, one issued each week, the prospectus tells us nothing.

A CURIOUS fact regarding a dragon-fly (*Æschna cyanea*, Müll.), often met with near Florence, has been observed by Signor Stefanelli. There were several nymphs of the animal in a cistern of water. Some which were near being transformed came out of the water a little way during the night, and, attacking several of the new-born perfect insects which had not yet begun to fly, voraciously devoured them. This singular practice (it is suggested) may explain why one finds such a small number of *Æschna cyanea*, in comparison with the number of nymphs. In raising the larvæ and nymphs of the dragon-flies, the best food, according to Signor Stefanelli, is meat, and especially fish.

THE Queenwood College Mutual Improvement Society seems to be an unusually active one. We have received a very favourable Report for the past term; the work done is very varied, and several of the lectures and papers have been published separately in a neat form.

FROM the Report of the Cardiff Naturalists' Society for the past year we learn that it is in a highly satisfactory condition.

A NEW work, entitled "The Hall Marking of Jewellery Practically Considered," by George E. Gee, author of "The Goldsmith's Handbook," "The Silversmith's Handbook," &c., is announced for immediate publication by Messrs. Crosby Lockwood and Co., London. The work will include an account of the Assay Towns of the United Kingdom, the Stamps at present employed, and will deal fully with the Laws relating to the Standards and the Marks at all the existing Assay Offices, &c. The same publishers also announce a new and enlarged edition of "The Manual of Colours and Dye Wares: their Properties, Applications, Valuation, Impurities, and Sophistications," revised and enlarged by the author, Mr. J. W. Slater.

A REVISED edition of the rules for the International Fisheries Exhibition to be held at South Kensington next year, has been issued. Among the prize essays are the following:—(1) 100*l*. The Natural History of Commercial Sea Fishes of Great Britain and Ireland, with special reference to such parts of their natural history as bear upon their production and commercial use. This would include natural history, habits and localities, fish frequent at different seasons, and artificial propagation. (This will not include the Salmonidæ.) (2) 100*l*. The effect of the existing National and International Laws for the Regulation and Protection of Deep Sea Fisheries, with suggestions for improvements in said laws. (3) 100*l*. On improved facilities for the capture, economic transmission and distribution of sea-fishes. Second-Class Prizes (amount not determined):—(1) On the introduction and acclimatisation of foreign fish. (2) On the propagation of freshwater fish, including Salmonidæ. (3) On oyster culture. (4) On the best means of increasing the supply of mussels and other molluscs (oysters excepted) used either for bait or food.

IN a recent paper called "Le Grain du Glacier" (*Arch. des Sciences*, April 15), Prof. Forel ably investigates the phenomena of glaciers, developing a theory similar to Hugi's and Grad's, and which he would rather designate by *increase of the crystalline grain*, than by *dilatation*. The crystalline grain is found to increase as the glacier descends: from the size of a small nut at the limit of the *nevé*, it becomes as big as a hen's egg at the terminal part; Prof. Forel has seen grains at the lower end of the Aletsch and other glaciers, 7 or 8 ctm. (over 3 in.) in the longer diameter. He estimates the volume-increase at about 4½ per cent. annually. Molecular affinity is the force which augments the crystal at the expense of the water which has penetrated the mass, circulating in capillary fissures. For details of the theory, and meeting of objections, we must refer to the original. *Inter alia*, Prof. Forel finds evidence against the view (which is adverse to the dilatation theory), that the central temperature of a glacier remains at zero, invariably and constantly. He considers the mean temperature of a glacier at the end of winter to be somewhat less than seven degrees below zero C.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. F. Foreman; a Collared Peccary (*Dicotyles tajaçu*) from South America, presented by Mr. G. H. Hawtayne, C.M.Z.S., a Mediterranean Seal (*Monachus albiventer*) from the Mediterranean, presented by Mr. M. Yeats Brown; an Oak Dormouse (*Myoxus dryas*) from Russia, presented by Prof. Wrzesniowsky; two Argus Pheasants (*Argus giganteus* ♂♂) from Malacca, presented by Major McNair, C.M.G., and Mr. J. M. Vermont; two Common Buzzards (*Buteo vulgaris*)

from Scotland, presented by Mr. J. Faed; a Great American Heron (*Ardea herodias*), captured at sea off Cuba, purchased; a Ruddy-headed Goose (*Bernicla rubidiceps*), bred in the Gardens. The following species of Butterflies and Moths have been exhibited in the Insect House during the past week:—Silkmoths: *Samia cecropia*, *Attacus cynthia*, *Attacus pernyi*, *Attacus atlas*, *Attacus roylei*, *Actias selene*, *Actias luna*, *Cricula trifenestrata*; Butterflies: *Papilio machaon*, *Anthocharis cardamines*, *Thais polyxena*, *Melitæa cinxia*; Moths: *Smerinthus ocellatus*, *Chærocampa elpenor*, *Proserpinus œnotheræ*, *Sesia scoliæformis*, *Sesia spheciformis*, *Trochilium apiforme*, *Sciapteron tabaniforme*, *Pygæra bucephala*. Twelve specimens of a leaf insect (*Phyllium scythe*) from eggs transmitted by Mrs. M. A. Meres and Mr. Wood Mason from India, have also emerged.

OUR ASTRONOMICAL COLUMN

THE TRAPEZIUM OF ORION.—Prof. Holden, in an appendix to the Washington observations for 1877, has discussed a long series of measures of the multiple-star Σ 748, made with the 26-inch refractor by Prof. Asaph Hall in 1877 and 1878. It is now known that the nebula in Orion was discovered by Cysat in 1618, thirty-eight years before Huyghens published an account of it, and his discovery is mentioned in his "Mathemeta Astronomica de Cometi Anni, 1618"; Bessel refers to it in his investigation of the elements of the great comet of this year, in the *Berliner Jahrbuch* for 1808. Cysat does not distinctly mention the number of stars, but clearly indicates their locality. Huyghens, in the "Systema Saturnium," 1659, describes his own discovery of the nebula, and refers to "three stars close together," which are shown in an accompanying figure. He saw the fourth star, completing what is now known as the trapezium of Orion on January 8, 1684, and Prof. Holden records that the last observation made by Huyghens was upon this system, on February 4, 1694, and the sketch in his manuscript journal under that date gives the four stars. In Hooke's "Micrographia," published in 1665, is a note (to which the attention of the American astronomer was drawn by Mr. H. B. Wheatley), which would imply that he was aware of the existence of the fourth star (notified by Cassini in his treatise on the comet of 1652), and of the fifth star, the discovery of which is usually attributed to W. Struve. He writes: "In that notable asterism also of the sword of Orion where the ingenious Monsieur Hugens van Zulichem has discovered only three little stars in a cluster, I have, with a 36-foot glass, without any aperture [diaphram] (the breadth of the glass being some three inches and a half), discovered five, and the twinkling of divers others up and down in divers parts of that small milky cloud." Sir John Herschel, in the *Memoirs* of the Royal Astronomical Society, vol. iii. mentions that Sir James South had pointed out to him in the original M.S. journals of the Royal Society a note which runs thus: "September 7, 1664 Mr. Hooke . . . the same relateth to have found those stars in Orion's belt, which M. de Zulichem maketh but three to be five." Prof. Holden made some special experiments in January, 1878, with the 26-inch refractor at Washington, the aperture reduced to 3½ inches, and arrived at the conclusion that if the fifth star were of the same brightness in 1664 as at this time, it would not have been discovered by Hooke; but, on the contrary, Mr. Burnham has brought together a number of cases in which the fifth star has been seen recently with such an aperture. The sixth star was detected by Sir John Herschel in 1830. Of other stars, suspected by several observers, Prof. Holden, during six years' observations of the nebula surrounding the trapezium, has not discovered any trace.

The Washington measures in 1877 were made in a dark field with the wires illuminated by a red-glass lamp; those of 1878 were made with the field illuminated, and with black wires. The mean results of the two years' observations of the four principal stars, after a complete reduction, are as follow, for the epoch 1878·0:—

	Position.		Distance.		Position.		Distance.
	°	′	″		°	′	″
ab ...	311	7·2 ...	13·118	*bc* ...	95	37·1 ...	21·758
ac ...	61	9·8 ...	13·454	*bd* ...	32	57·7 ...	8·774
ad ...	342	18·4 ...	16·773	*cd* ...	299	21·0 ...	19·364

The results obtained by South in 1820, W. Struve in 1836, Liaponoff in 1849, O. Struve in 1870, Nobile in 1876, and Jedrzjewicz for 1878, are brought together for comparison in Prof. Holden's paper.

Measures of the fifth and sixth stars in 1877–78, give the positions and distances subjoined, for 1878·0:—

		°	′		″
a and *a′*	...	121	25·2	...	3·984
a and *b′*	...	320	43·3	...	16·504
b and *b′*	..	352	8·0	...	4·194

In conclusion, Prof. Holden remarks: "It appears that after making due allowance for the unavoidable, accidental, and systematic errors, the comparison of all our measures on the six stars of this system shows their probable physical association."

THE COMET.—During the last fortnight the increase in the brightness of the present comet appears not to have differed sensibly from that indicated by theory. On May 21 it was hardly below 5·5m.

GEOGRAPHICAL NOTES

AT the Anniversary Meeting of the Royal Geographical Society on Monday, the medals were presented, as we said some time ago they would be, to Dr. Nachtigal and Sir John Kirk. Mr. Francis Galton gave some account of the progress of geographical teaching in schools, which the Society endeavours to promote by holding examinations and the grant of medals, &c. He quoted a passage from the report of the examiner, Prof. H. N. Moseley:—"I have," Prof. Moseley says, "to congratulate the society on the good work effected by its annual award of school medals. As my experience as an examiner in geography increases, the more I am convinced of its pre-eminent fitness as a subject of education, and the more I deplore that it is almost entirely neglected as such in this country. Competent teachers of the subject appear to be scarce indeed, but it is amply apparent from the society's examinations that most valuable results can be produced by really able instructors." This was the fourteenth year in which these examinations had been held, and fifty-six medals—four annually—had been awarded, while altogether ninety-eight boys had obtained honourable mention. Of fifty-two schools invited to compete, forty-one had sent up candidates. Among these the Liverpool School had been distinguished, its scholars having gained medals fifteen times; while Dulwich had obtained eleven medals since 1875, and two in each of the last three years. In the Scotch and Irish schools the boys were younger than in the high schools of this country, and that accounted, perhaps, for the fact that of five Scotch and seven Irish schools invited to compete, only two in each country had accepted the invitation. He regretted that the great schools of Rugby, Shrewsbury, King's College School, and St. Paul's School, London, had not yet sent competitors. The president then reviewed the progress of geography during the past year. He referred to various efforts which were being made to train those who might have opportunities of pursuing geographical research. Sir Allen Young, the president stated, was busy getting ready the whaler *Hope*, which he has hired, for the search for Mr. Leigh Smith and his party.

WE referred some weeks ago to the unusually early date at which ice appeared in the Atlantic this year; the supply has gone on unceasingly since, and the New York correspondent of the *Standard* states that the reports made by ships coming westward read like accounts of Arctic explorations:—One ship passed icebergs almost daily between May 7 and 17, in latitude 43 deg., longitude 37 deg. Many were of immense size, and were visible for forty miles, others were within arm's length of the ship's side. Arctic animals were seen upon them, some living, and others skeletons. The Atlas liner *Ailsa*, from Aspinwall, reports that in the middle of the afternoon of the 7th it was dark, and lights were necessary. Ten waterspouts were observed whirling in dangerous proximity to the ship. They were rendered visible by the lightning. The captain of her Majesty's ship *Tenedos* reports that the ice is nearly solid from Cape Breton to Newfoundland, and that two ocean steamers have been caught in it. The brigantine *Rescue* was completely crushed near Belle Isle. The crew, numbering seventy-two, took to the ice, although there was a heavy rolling swell surging among the floes. A perilous passage was made by the steamship *Mastiff*, of Scotland, which has arrived at Montreal She was among the ice for nine days. The crew and passengers, becoming desperate, cut a passage through the ice, which was sometimes twenty feet above the water. Another ship, the *Western Belle*, from Greenock, struck an iceberg off Newfoundland on May 1, and sank instantly with her captain (Frew) and thirteen hands.

HEFT V. of Petermann's *Mittheilungen* contains a long account, by Dr. Woeikof, of his journey in Mexico and Central

America; a paper of much interest by Dr. Konrad Jarz on the ice caves of Frain in Moravia; a short account, from the Russian of Fetisoff, of the Jashil Kul or Kulduk Lake in Central Asia (40° 45′ N., 76° 42′ E.); and some account of the Fiji Islands, to accompany an excellent new map of the group.

BARON NORDENSKJÖLD has published the first volume of the "Scientific Results of the *Vega* Expedition." It covers 800 pages with maps and tables. Besides the papers on the Aurora, of which we have already given an account, there are papers on the Health of the Expedition, the Colour Sense of the Chukchis, on the Botanical Collections, Meteorological Observations, the Invertebrata of the Arctic Seas, and other matters, by the various members of the expedition.

MESSRS. MACMILLAN AND CO. have published a sixpenny edition of Waterton's famous "Wanderings in South America," with the biographical introduction and explanatory index of the Rev. J. G. Wood, and 100 illustrations.

CAPT. BURTON and Commander Cameron have returned to England from their visit to the West Coast of Africa.

THE Geological Society of Stockholm will send a party to Spitzbergen this summer for the geological survey of that island. The two members selected for this object are the well-known geologists, Dr. Nathorst and Baron de Geer, who will leave Drontheim on June 1 in the whaling smack *Bjona* for their destination.

FOOD-PLANT IMPROVEMENT [1]

THE food question may be divided into two parts. 1. Its production (raw material). 2. Its preparation when produced. It is my intention to consider the first part only—food production. This, again, seems naturally to divide itself into: 1. Plant-food. 2. Animal food. And again, I propose to speak mainly of the first alone, alluding only incidentally to animal-food, upon which I will commence by making what remarks I have to make in order to clear the ground for the consideration of *plant*-food, the subject upon which I have been invited to address you. The improvement effected in the production of animal-food by the careful breeding or long repeated selection of sheep, cattle, and swine is so well known as to render it quite unnecessary to occupy much of our time in its consideration; I will only adduce one or two striking illustrations to show the *kind* of change which has been thereby accomplished. There is very strong ground for believing that the celebrated improved breed of shorthorn cattle is descended from a race originally black. Now black seems to have been in the eyes of all the best breeders of it a colour to be got rid of or wiped out, and this most certainly has been effected, for no single instance of it is now to be found. The improvement in the outward form of the animals has been carried almost to the breeders' ideal of perfection. These are external changes. Early in the history of shorthorns the breeders in Yorkshire made the production of milk their chief point, while those in Durham saved for breeding purposes the progeny of those cows only which showed the greatest tendency to lay on meat, and the result is the "Improved Durham," the pride and glory of the modern cattle show, but which are very poor milkers; while the "York" shorthorn is synonymous with a cow specially productive of milk. These are internal changes effected in *animals* by selection. When we turn to plants what do we find? The first thing, aud which is apparent to everyone, is that each produces "fruit after its kind." But close observation shows something more than this, viz. that, although each produces "after its kind," no two plants of any kind are absolutely alike. I speak not of monstrosities, of which the characteristics are not heritable, but of that ever present tendency throughout nature to variation, of which the horticulturist has availed himself. These variations, of which we can profit through the great principle of inheritance are generally slight, so much so, indeed, as to be quite inappreciable by the untrained eye or hand, but they are, nevertheless, striking enough to one competent to observe them. I will give a familiar illustration of this. Nothing can well seem more alike to an ordinary person than the sheep composing a well-bred flock, but the shepherd knows them all apart as well as if each had a name. To him they are no more "all just alike" than are the members of his own family. That these differences, apparently so slight, can be practically availed of, the existing improved breeds of sheep prove beyond doubt. I have already said that no two plants are absolutely alike. Of any two, then, one must be (in the direction of the difference between them) superior to the other. This fact, coupled with the principle of inheritance, is the very key-note of all possible plant-improvement. But, it may be asked, do plants offer opportunity of improvement by breeding equal to that presented by animals? Surely much greater. A cow or ewe produces at a birth one (or two) only—a single grain of wheat has produced a plant, the ears upon which contained 8000 grains all capable of reproduction. Now we can plant all these, and of the resultant 8000 plants reserve only the best one of all to perpetuate the race, rejecting every other. Can anything approaching such a choice as this be afforded any breeder of cattle or sheep, no matter how extensive his herd or flock? The advantage on the side of the wheat becomes almost infinite when it is considered that in the case of the above animals three years (instead of one) are required for each reproduction.

Before giving a few examples of results already obtained in cereal development, I will mention analogous improvements obtained in vines and in beetroot cultivated for sugar. Many years ago an old friend from Piedmont, having a relative a vine grower in Italy, carried back with him from here a sufficient knowledge of my system of selection to enable him to explain its principle. Some seven years after, upon my friend again visiting me, he told me that his relative, knowing him to be in London, had written to ask him if he could arrange there for the disposal of his wine, and that he, without reading this letter through, at once replied in the affirmative. This he did, as he knew the small extent of his relative's vineyard—some 12 acres. "You may judge of my astonishment," said this gentleman to me, "when upon reading his letter to the end, I found that he had, without having increased the extent of his vineyard, three times the quantity of wine he formerly produced, and this simply through having followed the plan of selection I had suggested to him." The cultivation of beetroot for sugar is a very important one, and any increase in the percentage of sugar contained in it is of very high value. The following from Toronto, Canada, appeared in the *Gardener's Chronicle and Agricultural Gazette* of March 22, 1873, under the head of "Foreign Correspondence":—"The most vital point, however, of the beetroot grower is the quality of the seed he uses; when beets were first grown for sugar, 5 per cent. of sugar was the amount obtained, now 15 per cent. is obtainable in favourable instances. This has been attained entirely by the improvement of the pedigree principle of the seed. The quality of richness in the root was attained by Vilmorin in the following manner:—Each root is a perfect plant, and therefore, in the examination of each root for the production of seed, the quality of it had to be ascertained. For this purpose, Vilmorin had a set of most delicate instruments made for the determination of specific gravity, and he found that the specific gravity was indicative of the sugar contained. The cups he used were no larger than a lady's thimble, and the saccharometer or measure of specific gravity equally small. The roots were first selected according to the best ordinary rules, then a small portion of each root was punched out of it in such a part as to injure as little as possible its future growth; the pieces were reduced to pulp, and the juice was extracted. All the roots which did not yield juice up to a certain standard were rejected, whilst those which reached the standard were planted for seed; the roots produced from this seed *were found to be constantly increasing in richness*, and a few years of the process produced the great percentage of sugar which is now attained." I may here mention in reference to the foregoing that I had, so long ago as 1860, come to the conclusion that vigour of vegetable growth was identical with the power of supporting animal life, and that specific gravity was the measure of both. The difficulty of determining the specific gravity of a grain of wheat without impairing its vital vigour was, however, found insurmountable.

I will now refer to results obtained in cereals by selection, taking wheat as the illustration. The chief points to attain are vigour of growth, hardiness, productiveness, and quality, and these have become as permanent characteristics of the pedigree cereals as are the good points of a thoroughbred animal, and reproduce themselves as surely. I begin with a report from near Perth, Western Australia, in 1862, nineteen years ago. "The English wheat (Hallett's) sown before I came, produced when drilled 29 bushels per acre; and when dibbled, 35 bushels per acre. The average crops about here are under ten; ours were six; and our neighbours' opposite 4½ bushels per acre. The

[1] Paper read by Major Hallett at the Brighton Health Congress.

largest ear produced 113 grains. The greatest number of ears on one 'stool' was 72." And next I give the *last* report received *of the same wheat*, from New Zealand, published in the Otago *Daily Times* of June 3, 1881:—"We have been shown two samples of wheat grown by Mr. M. C. Orbell, at Waikouaitai, and we do not remember to have seen any to compare with them in this country. They are known as Hallett's Pedigree wheat, Hunter's White, and Original Red. The yield exceeded anything ever grown in the district before. Mr. Orbell sowed 1¼ bushels upon one acre, and the yield was 72 bushels (or nine quarters) of good marketable wheat. Many of the plants consisted of over 90 ears, some of which contained as many as 132 grains each. Hallett's Pedigree white Canadian oats, introduced by the same importers, have, we understand, been cultivated by Mr. Shannan, of Conical Hills Station, with the same success as the wheat grown by Mr. Orbell." Thus, after 18 years (not without further selection, but the selection, having been continued annually at Brighton throughout the interval), the *same wheat* is found not only to have maintained, but to have further developed its vigour of growth, producing over "90" ears (instead of 72 ears) upon a plant, with 132 grains (instead of 113) in an ear. In England, 1876, 105 ears on a plant contained more than 8000 grains. (The average number of grains in an ordinary ear is 22 only.) From Essex in the same year as the date of the report first given, a crop of the *same wheat* was reported of 27 quarters on three acres, or nine quarters per acre, exactly the same quantity as that just given as obtained eighteen years later in New Zealand! Can illustration further go that there is no deterioration if only the selection be continued? Here is another experience in another year and country:—M. Tiéhonnais, editor of *La Revue Agricole de l'Angleterre*, writes October 9, 1865, from Brussières, France—"I am now staying here, a large farm where your wheat is extensively cultivated. The average this last harvest was at the rate of seven quarters to the English acre; the average of the other sorts in the same district did not exceed three quarters per acre."

Thus far as to vigour of growth and productiveness. I will now give examples of the other two points named, hardiness and quality. Report of the Minister of the Interior, Belgium: "I continue to sow the varieties of wheat improved by Hallett, above all the 'Red,' and 'Victoria' white. These varieties are very hardy. During the winter 1875 and 1876 many of our varieties of wheat have been destroyed by frost. The Hallett Red has successfully withstood the frost. It has been the same with the Victoria. On the other hand, the variety 'Galand' has been completely destroyed, not a single plant of it left. We have seen many fields of even our 'little red' variety, very hardy, which have greatly suffered." Lastly, as an example of sustained quality, a report from Linlithgow, Scotland, dated November 23, 1878: "I have again, making now ten years in succession, had the honour of topping the Edinburgh market with your Hunter's white wheat. I sent some of your barley to Australia, and in a few years it spread and gave immense satisfaction." The pedigree cereals having been grown in upwards of forty different countries in Europe, Asia, Africa, America, and Australasia, it is, of course, impossible to give, in such a paper as this, any idea of how widely *extended* has been the success of selection as exemplified in them, but I may mention that, in acknowledgement of that success, the Minister of Agriculture at St. Petersburg placed at my disposal the collection of all the agricultural colleges of Russia; and the Minister for Hungary sent through the Austrian Embassy at Vienna, and published, a most flattering communication showing results obtained by his Government by adopting my system. From Italy, Holland, Denmark, and Sweden, I have received similar acknowledgments. The Government of the United States published my system *in extenso* in the report for 1874 of the Department of Agriculture. The English Government, too, as will presently be seen, did me the honour to appropriate and apply my system in India.

A very practical acknowledgement has been made by less distinguished persons at home. When I commenced my system, now nearly twenty-five years ago, nothing had been done or attempted in the matter of the systematic improvement of food plants. One searches the advertising columns of the newspapers of that day without finding any of those announcements with which they now positively bristle, of seeds of all kinds, "of repeated selection," of "the latest selection," &c. But now many persons and firms, supposed to be of the highest respectability, and among them, as is always the case, some who ridiculed my work at the outset unblushingly try to identify their productions with my own, a sure and certain evidence that the reputation resulting from my system of selection has a very practical value.

In the case of the potato, next to the cereals in importance as a food plant, I have also applied my system, starting every year with a single tuber, the best of the year (proved to have been so by its having been found to produce the best plant), for now fourteen years. My main object here has been absolute freedom from disease, and these potatoes are now descended from a line of single tubers, each the best plant of the year, and absolutely healthy; and concurrently with the endeavour to wipe out all hereditary tendency to disease, I have always kept in full view the point of increasing productiveness. The result may be thus shortly stated. Dividing the first twelve years into three periods, the average number of tubers upon the annual best plant selected was, for the first period of four years, 16; for the second period of four years, 19; for the last period of four years, 27, or nearly double the number produced during the first series of four years. And if, as I might very fairly have done, I had confined the first period to the first three years (instead of four), the last period would have shown an average of 27 tubers against 13 in the first period, or more than double. Here, exactly as with the number of grains in the ear of the cereals, we reach in the last period of a long series of years a standard altogether higher than in the first years of the series, *and this no matter how we divide it into "periods."* In the latter "periods" of a series of years the results vary according to season and circumstances; but (except in a case of disaster) in no year of the last year of a series *do they drop back to the standard of the earliest years!* Can it possibly be conceived that all this is mere chance or accident? Is it not the fair conclusion, rather, that nature offers to us—nay, tempts us with—on every side rewards for intelligent observation, if we will only learn the lessons and avail ourselves of the variations which she presents to us?

I have hitherto spoken of food plants only, of vines, beetroot, cereals, and potatoes, but in a Health Congress such as this, I may be permitted also to refer to plants destined for clothing; of little, if of any, less importance than food to the health of mankind. I will take the cotton plant as an illustration. In the *Times of India*, November 6, 1869, an article headed "Cotton Report" says: "The Cotton Administration Report for the past year concluded with an interesting notice of the experiments made last season and of others which are now in progress in different parts of the Presidency, for growing cotton of an improved quality. To those who remember the conclusions recorded by Mr. Walter Cassels, in 1862, in his work prepared and printed on account of Government, it may seem strange that such experiments are now undertaken at all. These conclusions, drawn from the past history of cotton cultivation in Bombay, were (1) that 'exotic cotton cannot be successfully cultivated on a large scale in Bombay Presidency, except in a limited portion of its southern districts'; (2) that 'Indian cotton may be improved in cleanliness and somewhat reduced in cost, but the general characteristics of the staple will not be materially altered.' Because lacs of rupees had been in a long course of years expended in cotton experiments, and these had resulted in a long list of failures, it seems to have been supposed that the utmost had been tried in vain, and that the question had been finally set at rest." The article, having referred to Mr. Cassel's opinion that the failure of exotic cotton when cultivated on a large scale was due to the violence of the Indian season, continues thus:—"The climate of Hindostan is, we admit, in nearly all that relates to cotton, very different to that of any but the most arid districts in our Northern Deccan collectorates. But it is plainly a fallacy to attribute to climatic influence results for which other causes can be found independent of the climate, and, unlike the climate, quite within our control. One of these causes is indicated in a Minute by the Governor of Bombay, dated January 10, 1869, in which his Excellency, who attaches great importance to the subject as one 'of vital interest to this Presidency,' remarks that 'the experiments that have hitherto been made by the order of Government with a view to improvements in the cultivation of cotton, do not appear to have been hitherto carried out with sufficient persistence or sufficient method. So that, in fact, as remarked in the report before us in the matter of Indian cotton improvement, we are yet but on the threshhold of our experience, but let us hope that the course will now be distinctly mapped, and that we may be saved from the task of beginning our experience again and again. What is still wanted, not

only in the North-West Provinces and Upper India, but in the far more favoured cotton fields of our Presidency, is an adequate testing and full authentication of some inexpensive method of treatment or cultivation, which shall be equally applicable to the exotic, hybridi ed, and good indigenous varieties, and which the ryots themselves will be able to appreciate alike under their present simple methods of tillage, or under any improved system they may eventually be induced to adopt. There is at last, we think, some prospect of this *desideratum* being attained. The minute of his Excellency suggests more than simply a systematic method of operation in future experiments. It describes what is known at home as *Hallett's pedigree system*, which consists in the selection by hand of the finest seed from each successive year's crop, and the annual reproduction of the plant only from such seed; and it enjoins the adoption of this plan in experiments both with exotic and indigenous cotton, as the best means of acclimatising the one and improving the other. The advantage of this system appears so very manifest that the wonder seems to be that it has never yet been tried. A cultivator selecting the finest bolls in his field of cotton, and putting them aside, extracting from them at leisure the seed for his next sowing, is a thing that has never yet been heard of; but the matter is so simple, so reasonable, that we have little doubt that the system will be generally adopted when the ryots come to be acquainted with it, and its advantages are explained to them." The same article then goes on to say: "The pedigree system was begun last year in different parts of the Presidency, but cannot be said to have yet had to any appreciable extent a trial, as it is obvious that the effect of it can only be judged by the character of the produce of successive years. In the experiments now being conducted in accordance with the plan suggested by His Excellency, there is yet another element of success in the efficient character of the agency employed. The Cotton Departments are assisted in the work by four practical horticulturists, Messrs. Shearer, Stormont, Strachan, and Milne, who have been sent out to this country for the purpose by the Secretary of State, who, we believe, selected them from a number of applicants on the recommendation of Dr. Hooker, of the Botanical Gardens at Kew." I wrote to my friend Sir Joseph Hooker, who, in reply, says the men were sent out from Kew in 1869, but that he has no statement of the results beyond a newspaper cutting, stating that their services were highly approved of, adding, "cotton is coming down from the country much better in quality and in much larger quantities." I therefore wrote to the India Office requesting to be furnished with a copy of the Minute above referred to, and with information as to the exact plan adopted and the results obtained. I can only suppose that there is some difficulty in doing this, as, although I stated that these particulars were required for the Congress this day, they have not yet reached me.

Had the Government, when thus appropriating and applying my system, done me the honour to consult me upon it, I should have pointed out that mere horticulturists, however skilful, would not (unaided) be likely to accomplish very much. It appears that in India there are thirty different kinds of cotton grown, in as many separated districts, for the Liverpool market. In each district the kind of cotton grown there is said to be that most suitable, and indeed the only kind that can be cultivated there with advantage. If this be so, then there must be thirty selectors—one in each district—in order to improve to the utmost the cotton most suitable to it. I do not profess any special knowledge of the growth of cotton, but I know something of the growth of wool, and I apprehend that fineness, and length and strength of fibre are qualities equally desirable in both. I have seen a buyer of wool, when blindfolded, tell by the touch the age and sex of the animal from which the fleece in his hand came, and I have tested beyond all possibility of doubt his ability to do this. I am told there are men in Liverpool who have an equal gift in judging cotton, but that such men soon make their fortunes there. But these are exactly the men who are wanted for cotton selectors in India. The available differences of plants are slight, and when out of a number the selection has reduced the competing plants to two or three, the difference is very slight indeed, but still very real. With many different points to take into account, I have occupied weeks in studying the final best two plants. It is evident that if there is anything at all in selection, a selector, ignorant of the one thing needful, may pedigree in the wrong direction, as the first Napoleon did unconsciously when his conscriptions left only those men who were quite impossible for soldiers to be progenitors of the future Frenchmen with the result of the standard in the army having to be lowered by five inches. I must not, I suppose, be surprised if the Government has imperfectly understood my system when such a man as Mr. Darwin, in his "Cross and Self-fertilisation of Plants," can thus write of it:—"Loiseleur-Deslongchamp (Les Céréales) was led by his observations to the extraordinary conclusion that the smaller grains of cereals produced as fine plants as the large. This conclusion is, however, contradicted by Major Hallett's great success in improving wheat by the selection of the finest grains." Here finest evidently means largest; but size of grain is not even an element in my system of selection.

If then we can seize upon these variations in plants, and by means of the principle of inheritance, perpetuate, increase, and accumulate year by year the original variation in the desired direction, what a field does it open to us for increasing this world's plant food! And how vast is this field compared with that presented by the food-producing animals, in mere number probably not equal to the food-plants upon a single English farm; for while these animals supply food for man alone, and for him only in part, plants may be said to almost wholly support both them and man. Vast, indeed, may this field be called, for it includes not only the plants destined for food and clothing, but also every kind of plant which contributes to the welfare and happiness of mankind; surely a field, then, worthy of any man's labour!

Since this paper was read a Minute by His Excellency, Sir Seymour Fitzgerald, the Governor of Bombay, dated January 10, 1868, has been sent to Major Hallett by the direction of the Secretary of State for India, together with reports extending to 1870 only.

"In England I have had opportunities of seeing on my own land, and on the properties of other gentlemen, how much can be effected in the improvement of cereals by a continued attention during successive years to the selection of the best seed only from crops of a common variety. The pedigree wheat, which bears the name of Mr. Hallett, a Sussex gentleman, is, in fact, a new variety which he has produced by the constant selection each year of the finest ears produced on his farm near Brighton, and by his never permitting any seed from small or inferior ears to be sown. None but the best ears selected by hand were set aside the first year for seed; from the produce of these the best were again in the same manner selected by hand, and this course was continued for several successive years; the final result was the introduction of Hallett's Pedigree Wheat, which I have known in my own experience to produce a crop nearly 50 per cent. more in quantity, and 50 per cent. more valuable in quality, than that produced from the best seed that could be purchased in the market, and this in the same field, under exactly the same circumstances, and with the same care taken in the cultivation.

"I believe the same result may probably be obtained if the same process is adopted with our indigenous cotton. At any rate, I desire the experiment to be carefully made, and will take care that funds are placed at the disposal of the Inspector-in-Chief for this purpose. The experiment should be tried not only in different districts but in several parts of each district, and a sufficient breadth should be sown in each case to ensure a fair and satisfactory trial.

The Inspector-in-Chief is, therefore, authorised to make the same experiments as those I have suggested as to the indigenous cotton—with all the exotic varieties he may receive—in the same manner and on the same scale. Even if they are not successful to the extent and in the manner I anticipate, they will serve to show us, if carefully continued for the next three or four years, what are the exotic varieties of cotton which we can with confidence encourage the cultivators in each district to adopt, as being best suited to the particular circumstances of their lands."

The following extract from Administration Report, Cotton Commissioners' Department, for the year 1870-71, was received by Major Hallett on January 9, 1882. Major A. T. Moore, Acting Cotton Commissioner and Inspector in-Chief, writes under date Bombay, October 31, 1871, on the advantages of "Selection":—

"Taking everything into consideration, I think the fact of the heavier yield—by more than double—being in favour of the 'Pedigree,' goes to show that 'selection,' as desired by His Excellency Sir Seymour Fitzgerald, should be carefully carried out; that the cultivators should be supplied from the Government crops with as much seed as possible, and at the same time, that the necessity for selection should be earnestly pressed on

their notice; while the Superintendents themselves, by carefully and steadily pursuing the same plan year by year, by selecting from all their crops, and again selecting from that selection, will be able apparently, if the present results may be relied on, to increase the production and fruitfulness of the plant, and in the course of a few seasons to establish a veritable 'Pedigree Cotton,' as unlike its parent as the 'English thorough-bred,' with his long stride and fine skin, is unlike the stock whence he originally sprang. It remains for me to notice the avidity with which our surplus seed was purchased by the cultivators. Mr. Wilkinson says this seed was sufficient for the requirements of two villages, and that the crop produced was an abundant one. He further adds, 'I was informed by the Patel of one of the villages that this seed had given great satisfaction; yields being reported of 96 lbs. to 150 lbs. cotton per acre, according to the amount of care in cultivation.' This gives an average of 123 lbs., but I will only take 100 lbs. as the *average* product, and even then I find the figures loudly speaking in favour of carefully picked and selected seed.

	Per acre. lbs.
Average yield of our Departmental seed ...	100
Average yield for Kandeish	$82\frac{5}{8}$
Difference in favour of our seed	$17\frac{3}{8}$
	or about 20 per cent.

If only this 20 per cent. could be established as the increased out-turn, by the efforts of our Department, it would bring wealth to thousands, and unspeakable benefit to the Presidency generally. It would represent an increased produce, valued at last year's rates, of Rs. 26,365,979 = £2,636,597 18*s.* 0*d.*; a result and a prize worth striving for, and, it would appear, possible of attainment!"

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The election to the Professorship of Animal Morphology will take place on May 31.

The Moderators and Examiners for the Mathematical Tripos have announced that logarithmic tables will be provided for each of the candidates during the examination.

The mineralogical laboratory will be open to students during July and August.

The proposed enlargement of the space available at the new museums for Practical Morphology and Histology is to be at once proceeded with.

Mr. W. H. Caldwell, B.A., Scholar of Gonville and Caius College, is approved as a Teacher of Comparative Anatomy with reference to certificates for medical study.

Dr. Anningson has been approved as a teacher of Medical Jurisprudence in the Medical School.

The proposal to continue the opening of the Botanic Garden for three hours on Sundays to Members of the Senate accompanied by their friends during the summer months, has met with warm opposition from some who consider that in this case Sunday labour is imposed on others for the selfish pleasures of a few. It has been pointed out that owing to the value of the contents of the garden it must always be watched, and it could not possibly be said that the proposed regulations will impose additional Sunday labour. The voting on this question takes place to-day (25th).

LONDON.—Prof. Ray Lankester has been re-appointed Professor of Zoology and Comparative Anatomy in University College, London.

SCIENTIFIC SERIALS

American Journal of Science, May.—Photographs of the spectrum of the nebula in Orion, by H. Draper.—Mean annual rainfall for different countries of the globe, by A. Woeikoff.—Physiological optics, by W. L. Stevens.—Flood of the Connecticut River valley, from the quaternary glacier, by J. D. Dana.—Brazilian specimens of Martite, by O. A. Derby.—Method of determining the flexure of a telescopic tube for all positions of the instrument, by J. M. Schæberle.—Dykes of micaceous diabase penetrating the bed of zinc ore at Franklin furnace, by B. K. Emerson.—Occurrence of smaltite in Colorado, by M. W. Iles.—Conditions attending the geological descent of some freshwater gill-bearing molluscs, by C. A. White.—Measures of the rings of Saturn in the years 1879, 1880, 1881, and 1882, by E. S. Holden.—Interference-phenomena in a new form of refractometer, by A. A. Michelson.—New minerals, monatite and monite, with a notice of pyroclasite, by C. U. Shepard.—Marine fauna of New England, by A. E. Verrill.

Journal of the Franklin Institute, May.—On the several efficiencies of the steam-engine, and on the condition of maximum economy, by R. H. Thurston.—Ninety miles in sixty minutes, by W. B. Le Van.—Intonation of chime bells, by J. W. Nystrom.—The Mears chlorination process, by W. U. Greene. Action of charcoal on a solution of gold chloride, by G. E. Kœnig.

Bulletin de l'Academie Royale des Sciences de Belgique, No. 3. —On the sensations the author experiences in his eyes, by M. Plateau.—On a claim of priority, introduced in the Academy by M. E. Dewalque, regarding my note on the origin of Devonian limestones of Belgium, by M. Dupont.—On the respiratory effects of excitation of the pneumogastric, by M. Henrijean.—Various products obtained from fresh stocks of peony; new reaction of salicylic acid, by M. Jorissen.—Reports.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol xv. fasc. vii.—The geology of the Parman Apennines, by A. Del Prato.—The double quadratic transformation of space, &c. (concluded), by F. F. Archieri.—On rational skew curves, by L. Weyr.—On the transformation of the co-ordinates in space, by F. Borletti.

Fasc. viii.—On a formula of Cauchy, concerning the development of functions in infinite products, by P. Cazzaniga.—Whether cemeteries may have an injurious influence on the public health, by L. Gabba —Remarks on the subject, by C. Zucchi, and reply by L. Gabba

Atti della R. Accademia dei Lincei, vol. vi., fasc. 10.—On some derivatives of citraconic acid, by Drs. Ciamician and Dennstedt.—Studies on fluoxysalts and fluosalts of molybdenum, by Signors Mauro and Panebianco.—Reports.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 4.—"On the Specific Resistance of Mercury." By Lord Rayleigh, F.R.S., Professor of Experimental Physics in the University of Cambridge, and Mrs. H. Sidgwick.

The observations detailed in the paper were made with the view of re-determining the relation between the B.A. unit and the mercury unit of Siemens, *i.e.* the resistance of a column of mercury at 0°, one metre in length, and one square millim. in section.

According to Siemens' experiments

1 mercury unit = 0·9536 B.A. units,

and according to Matthiessen and Hockin,

1 mercury unit = 0·9619 B.A. units.

The value resulting from our observations agrees pretty closely with that of Siemens. We find—

1 mercury unit = 0·95418 B.A. units.

Four tubes were used to contain the mercury, of lengths varying from 87 to 194 centims. The diameter of the three first tubes was about 1 millim., and that of the fourth about 2 millims. The final numbers obtained from the several fillings of the tubes are as follows:—

Tube I.	0·95386 0·95412 0·95424 0·95436 0·95421	... ... 0·95416
Tube II.	0·95389 0·95414 0·95437 0·95436	... ... 0·95419
Tube III.	0·95424 0·95418 0·95399 0·95425	... ... 0·95416
Tube IV.	0·95440 0·95415	... ... 0·95427

Combining the results of the present paper with our determination of the B.A. unit in absolute measure, we get—

1 mercury unit = 0·94130 × 10^9 C.G.S.

Chemical Society, May 18.—Dr. Gilbert, president, in the chair.—The following papers were read:—On the precipitation of the alums by sodic carbonate, by E. J. Mills and R. L. Barr. The authors have determined the quantity of alumina precipitated in one hour from a solution of potash alum containing 1 per cent. of sulphate of alumina by varying quantities of sodium carbonate solution. The precipitation takes place in three stages: in the first no precipitation occurs—at the end of this stage, the ratio is 1 molecule of aluminium sulphate to $\frac{3}{5}$ of a molecule of sodium carbonate; during the second stage precipitation is continuous—at the end of this stage about $\frac{1}{3}$ the alumina is precipitated, the ratio, 1 molecule of the sulphate, to $\frac{3}{2}$ molecule of the carbonate; at the end of the third stage the precipitation is complete, the ratio being 1 molecule of the sulphate to $\frac{12}{5}$ molecule of the carbonate. Similar results were obtained by precipitating potassiochrome alum.—On rotary polarisation by chemical substances under magnetic influence, by W. H. Perkin. The author has determined and compared the power which various organic liquids have of rotating the plane of polarisation, when under the influence of an electro-magnet; and he has calculated the rotary power possessed by the columns of liquids, which would be formed, by the condensation of unit-columns of their vapours, or, in other words, the rotary power possessed by lengths proportional to molecular weight. The numbers thus calculated clearly indicate that the molecular magnetic rotary power increases *pari passu*, with each increment of CH_2.—On the constitution of Amarin and Lophin, by F. R. Japp and H. H. Robinson. By the action of parahydroxy-benzaldehyde upon benzil in presence of ammonia, the authors prepared a substance having the formula of hydroxylophin, which by distillation with zinc dust furnished crystals resembling in all respects the lophin prepared by Laurent, Fownes, &c. Lophin, therefore, belongs to Hübner's anhydrobases, and is an anhydrobenzoyldiamidostilbene.

Geological Society, May 10.—J. W. Hulke, F.R.S., president, in the chair.—Arthur Leech was elected a Fellow, and Prof. L. Rütimeyer a Foreign Member of the Society.—The following communications were read:—On the relations of *Hybocrinus*, *Baerocrinus*, and *Hybocystites*, by P. Herbert Carpenter, M.A. Communicated by Prof. P. Martin Duncan, M.B., F.R.S., V.P.G.S.—On the Madreporaria of the inferior oolite of the neighbourhood of Cheltenham and Gloucester, by R. F. Tomes, F.G.S.—On the exploration of two caves in the neighbourhood of Tenby, by Ernest L. Jones. Communicated by Prof. W. Boyd Dawkins, F.R.S., F.G.S. The caves noticed in this paper were that of Coygan, near Laugharn, partially described by Dr. Hicks in the *Geological Magazine* in 1867, and a cave known as Hoyle's Mouth, reported on to the British Association in 1860 by the Rev. Gilbert N. Smith. Both caves were rock-fissures. The Coygan cave had been a hyæna-den, as was shown by the deposits of crushed bones and coprolites trodden down into a solid mass by the passing of the animals. Besides remains of hyæna, it furnished those of horse, mammoth, tichorhine rhinoceros, elk, red deer, roe deer, reindeer, cave bear, cave lion, *Bos primigenius*, wolf, and fox. The presence of hippopotamus was doubtful. Besides these animals, the presence of Palæolithic man in the cave was indicated by some cut bones, and by two flint-flakes evidently chipped by man. In the second cave, Hoyle's Mouth, the hyæna, the cave bear, &c., were wanting, the place of the latter being taken by the common brown bear. In one part, remains of an old hearth were found; and the whole contents of the fissure pointed to a Neolithic date. At one time the cave appears to have been used as a place of sepulture.—Note on the comparative specific gravities of molten and solidified Vesuvian lavas, by H. J. Johnston-Lavis, F.G.S. From some experiments made on Vesuvian lava, Prof. Palmieri, in 1875, expressed the opinion that its specific gravity, when molten, might be as high as 5·0, though when cooled it is only 2·7. The author described the results of experiments made in December, 1881, on some lava flowing across the Atrio del Cavallo. Favourable circumstances enabled him to gain a position above a perfectly molten stream, the surface of which was protected from radiation by the heated walls of a tunnel which the lava had already formed by cooling of the crust. On to this were dropped, from a height of $1\frac{1}{3}$ yard (*a*) light scoria; this floated on the surface until lost to view (the stream could be watched for 150 yards or so); (*b*) fairly solid lava, with some vesicular cavities: this slowly sank, until after some distance it disappeared; (*c*) the most compact lava that could be found, in which, however, were a few small cavities; this sank rapidly, the molten rock welling up round it. The author considered that these experiments demonstrate that the cooled lava is more dense than the molten, and that the apparently contradictory results obtained by Prof. Palmieri were due to the fact that the surface of the stream, by loss of heat, had become viscid, so that the solid material floated, though of greater density. The author concluded by citing other confirmatory evidence of his view.

Entomological Society, May 3.—Mr. H. T. Stainton, F.R.S., president, in the chair.—The president alluded to the interest which the late Mr. C. Darwin, who was one of the original members of the Society, had always shown in entomology.—The Secretary read a communication from the Secretary of the Essex Field Club, relative to the scientific importance of Epping Forest being preserved in its *natural condition* "*unimproved*," and requesting the members to join in a Memorial to the Conservators to this effect, lest it should be converted into a mere park.—Exhibitions: Varieties of *Fidonia atomaria* and *Anchocelis pistacina*, by Mr. W. C. Boyd; a male of *Cryptus titilator*, by Mr. T. R. Billups; a hybrid between *Antheræa Perryi* and *Roylii*, by Mr. W. F. Kirby; and a curious abnormal growth of the flowers of the ash (produced by a gall-mite), by Miss Ormerod. Mr. E. A. Fitch called attention to a woody spherical gall on ash keys, produced by a curmlionideons (?) larva.—Papers read: Further additions to Mr. Mashall's Catalogue of British *Ichneumonidæ*, by Mr. J. B. Bridgman; a continuation of his synopsis of British Hymenoptera, by Mr. E. Saunders; and on the supposed abnormal habits of certain species of *Eurytomidæ*, a group of the Hymenopterous family *Chalcididæ*, by Prof. J. O. Westwood.

Meteorological Society, May 17.—Mr. J. K. Laughton, F.R.A.S., president, in the chair.—Miss W. L. Hall, Mr. E. J. Pearson, Dr. J. R. Somerville, and Mr. W. J. V. Vandenbergh were elected Fellows of the Society.—The following papers were read:—On the diurnal variation of wind and weather in their relation to isobaric lines, by the Hon. Ralph Abercromby, F.M.S. By constructing synoptic charts at different hours of the same day, and by comparing the wind and weather records at the different hours, and examining their relation to mean curves of diurnal variation, the author shows that the mean diurnal increase of the wind's velocity is explained by the fact that for the same gradient there is more wind by day than there is by night. The mean diurnal veering of the wind is explained by the fact that in cyclones the wind is a little more incurved, and in anticyclones a little more outcurved, by night than by day. The mean diurnal increase of the frequency of rain during the day hours is explained by the fact that in any given cyclone the area of rain is larger by day than by night. The diurnal changes of every element are superimposed on the larger general changes, and are independent of each other. Great stress is laid on this point, both as explaining and classifying many meteorological questions, and as simplifying the problem of weather forecasting. The author gives a simple hypothesis, from which it appears that the diurnal veering and increase of rain follow as a natural consequence of the diurnal increase of velocity.—Mechanical conditions of storms, hurricanes, and cyclones, by W. F. Stanley, F.M.S.

Sanitary Institute of Great Britain, May 17.—Annual General Meeting.—Prof. F. S. B. F. Dechaumont, M.D., F.R.S., in the chair.—A favourable report was presented by the council on the progress made by the Institute during the past year. The chairman gave an address, and the officers for the ensuing year were elected, the President being His Grace the Duke of Northumberland, K.G., and the trustees Sir John Lubbock, Bart., D.C.L., F.R.S., Dr. B. W. Richardson, F.R.S., and Thomas Salt.

Institution of Civil Engineers, May 16.—Sir Frederick Bramwell, vice-president, in the chair.—The first paper read was "On the various systems of grinding wheat, and on the machines used in corn-mills," by Mr. W. Proctor Baker.—The second paper was on "Modern Flour-milling in England," by Mr. Henry Simon.—The third paper was on "Roller-mills and milling as practised at Budapest," by Mr. W. B. Harding.

EDINBURGH

Royal Society, May 15.—Prof. Balfour, vice-president, in the chair. Mr. Murray read an account of the explorations which had been carried out by Staff-Commander Tizard and himself in the Faroe Channel during the summer of 1880. In

H.M.S. *Knight Errant* they had taken a series of soundings and dredgings with the view of testing the truth of the theory that a barrier stretched across between the North-West of Scotland and the Faroe bank, separting the cold and warm deep-sea areas which previous exploration had shown to exist in close contiguity to each other. In this they had been quite successful, proving that there was a narrow barrier separating the northern cold area from the southern warm area. From the specimens of rock obtained from the top of this ridge, they concluded that the ridge was in all probability an ancient moraine. The objects, animal and otherwise, brought up from the bottom had been examined carefully by various scientific men, and the paper consisted in great part of their report—sixteen in all.—Mr. E. Sang, in a short notice of the solar eclipse of May 17, laid before the Society calculations which so supplemented for Edinburgh the times and phases given in the *Nautical Almanac* as to make the comparison between calculation and observation more accurate. Should the morning prove favourable for observation, he hoped to be able to lay before the Society the result of the comparison.—Prof. Tait communicated a paper by Mr. A. P. Laurie, on a new secondary cell, with which he had made a long series of experiments. The cell consisted of two copper poles dipping into chloride of zinc, and was charged in the usual manner by running a current through it. Zinc was deposited on the one pole, and cuprous chloride was formed at the other. Even with the small sized cells which were used, the results obtained were tolerably satisfactory. They suffered greatly from loss, however, being in this respect in no way superior to the other known forms of secondary cells.

PARIS

Academy of Sciences, May 15.—M. Jamin in the chair.—The following papers were read:—Observations of small planets with the great meridian instrument of Paris Observatory during the first quarter of 1882, by M. Mouchez.—New note on the project of formation, in Algeria and Tunisia, of a so-called interior sea, by M. Cosson. He brings forward a series of objections to the scheme.—Reply to M. Cosson's note, by M. de Lesseps.—M. Alph. Milne-Edwards presented, in his own name, the second volume of text, and vols ii. and iii. of plates (266 in number) of "L'Histoire Naturelle des Oiseaux de Madagascar." This raises to 400 the number of plates of birds.—Spiraloid drums for cables of equal resistance, by M. Haton de la Goupillière. This relates to extraction from mines. The first part treats of the general properties of every system, of rigorous equilibrium, whatever the form of the cable (cylindrical from end to end, formed of successive cylindrical parts, conical, logarithmic, &c.). In the second part, the general properties arrived at are employed to determine the drum of equilibrium in the case of the logarithmic cable, which represents the exact form of equal resistance. Simple formulæ are furnished for the radii of winding.—Synthesis of several organic compounds by means of electrolysis of water, of acid, alkaline, and alcoholic solutions with electrodes of carbon, by MM. Bartoli and Papasogli. They electrolysed distilled water during about six weeks, using a strong battery (1200 D) the first two days, then 100 Bunsens for ten days, then twenty Bunsens for thirty, the electrodes being carbon. Mixed with the disaggregated carbon was found a dark matter, which they call *mellogen*, because, in oxidation, it produces the acids of the benzocarbonic series. Its other properties are described. Using alkaline solutions (hydrates or carbonates) as electrolytes, the authors got a good deal of mellic acid and very little mellogen; the reverse being the case where the electrolyte was acid.—On the spherical representation of surfaces, by M. Darboux.—On the conditions of achromatism in phenomena of interference, by M. Hurion. He gives an experimental verification of a principle enumerated by M. Cornu. In a system of interference fringes from heterogeneous light giving a continuous spectrum, there is always an achromatic fringe which plays the part of central fringe, and is found where the most intense radiations present a maximum or minimum difference of phase.—Aperiodic galvanometer of MM. Duprez and d'Arsonval. This is for very weak currents. Between the poles of a horse-shoe magnet set vertically, is a rectangular frame wound with fine wire, connected by two wires, of silver or copper, with a bent support above, and an elastic slip below. These wires, whose tension is regulated by screws, are axial to the frame, to which they also bring the current. The upper wire has a mirror at its lower end; and within the frame is supported an iron tube to strengthen the magnetic field. The authors indicate a method of graduating galvanometers.—On the length of sparks of the discharge of an electric condenser, by M. Villari. When a condenser is discharged by making it produce one spark or two, the length of the first is not equal to the sum of the lengths of the others, and the sum of lengths of the sparks is not always constant. Small sparks have the effect of elongating another produced at the same time in the circuit; and this influence grows with the charges of the condenser. It is connected with a sensible diminution of the interior discharge, and increase of the exterior.—Existence of lithine and boric acid in notable proportions in the waters of the Dead Sea, by M. Dieulafait. In a cubic centimetre there is enough lithine to distinctly show, at least a thousand times, the spectrum of that substance. The boric acid can be practically recognised with the product of a single cubic centimetre. These facts (contrary to previous ideas) prove the marine origin of the Dead Sea.—On the laws of solubility of carbonic acid in water at high pressures, by M. Wroblewski. The temperature remaining constant, the coefficient of saturation increases much less quickly than the pressure, while tending to a certain limit. The pressure remaining constant, the coefficient increases when the temperature diminishes.—On the mechanism of putrid fermentation of proteic matters, by MM. Gautier and Etard. The acid fermentation which arises in a few days is an epiphenomenon, not necessary, and not affecting the albuminoid molecules.—On a case of isomerism of bichlorocamphor, by M. Cazeneuve.—On purpurogalline, by MM. de Clermont and Chautard.—On the dimorphism of stannic acid, by MM. Levy and Bourgeois.—On chronic poisoning by arsenic, by MM. Caillot de Poncy and Livon. Cats receiving arsenic in small doses from time to time, eat more, and fatten, for a time, showing every sign of good health; but by and bye they grow lean, have diarrhœa, lose appetite, and become languid; and they die in an anæmic and lean state. The authors describe the changes (fatty degeneration) in the lungs, and mesenteric ganglions.—On a disease of early beans in the environs of Algiers, by M. Prillieux. A parasitic champignon produces white wadding-like tufts on the plant.—M. Laussedat said he had seen Mercury with the naked eye on May 11, at 8 p.m.

VIENNA

Imperial Institute of Geology, April 18.—The following papers were read:—C. Doelter, on pyroxenite, a proposal for the classification of the eruptive rocks.—V. Hilber, geological mappings of Zolkiew and Rawa-Ruska (Gallicia).—Th. Fuchs, which deposits are to be considered as of deep-sea origin?

May 2.—T. N. Woldrich, contributions to the fauna of the Istrian breccia.—R. Zuber, geological notes from the Carpathian Mountains of Eastern Gallicia.

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